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PROGRESS OF MEDICINE DURING THE PAST TWENTY-FIVE YEARS AS EXEMPLIFIED BY THE HARVEY SOCIETY LECTURES[1]

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THE constitution of this society states its object to be the diffusion of knowledge of the medical sciences, or, more specifically, "the diffusion of scientific knowledge in selected chapters in anatomy, physiology, pathology, bacteriology, pharmacology and physiological and pathological chemistry." This statement implies that these sciences form the foundation on which the superstructure of medicine is built. That medicine itself is omitted from this catalogue of sciences suggests that medicine is something different, that as an independent branch of human knowledge it does not exist, or, if so, that its content and the methods for its pursuit are not of a character to justify its inclusion in this family of sciences. Time would not permit me, even if it were profitable, to discuss the justification for this attitude, but I may point out the great and important change that has taken place in the past twenty-five years toward this point of view. The independent position which this discipline now occupies in certain universities, its elevation to a rank equivalent to that of the other sciences mentioned and its disinterested pursuit by men whose chief object is its advancement indicate one of the most striking changes which has occurred in medicine, and to-day, if the constitution of this society were to be written, its object would probably be stated to be the diffusion of scientific knowledge in medicine and related sciences.

[1] Address given at the celebration of the twenty-fifth anniversary of the founding of the Harvey Society, at the Academy of Medicine, New York City, May 15, 1930.

For this reason, as a humble disciple in this new science, yet one of the oldest, I feel gratified in being asked to discuss briefly the changes that have taken place in it during the past quarter century. Possibly the simplest way to approach this task would be to analyze carefully the entire series of lectures, pick out the new facts, or apparent new facts, presented by each one of the speakers, carefully catalogue and index and group them, possibly give them a statistical treatment, and then present to you my results, and conclude with an apotheosis of modern science, particularly of those sciences in which we are interested, not forgetting to point out the great and beneficent practical results that have been attained. I have preferred, however, to consider this subject in a somewhat different manner, and if I sometimes seem to strike a critical note I trust you will remember that I have endeavored to consider my subject in a purely objective and disinterested manner, as befits this society.

The historian of an epoch is usually granted a retrospection of a sufficiently distant past that he can discriminate between the momentous events of the period and the less significant details which are apt to be magnified in the eyes of contemporaries. For one who has lived in the midst of events to attempt, at the end of so short a period as twenty-five years, critically to survey that period is a hazardous undertaking. Moreover, no period in history can be satisfactorily isolated from that which precedes and follows.

That changes in concepts are constantly occurring, new facts being brought to light, in medicine, as elsewhere, is obvious to all. What we are considering, however, is not change but progress. With the exception of a few philosophers, people of to-day believe in progress. It is almost axiomatic. But man did not always accept that assumption. The Greeks kept looking to the past as the halcyon days and longed for their return. It was only at the end of the eighteenth century, when the multiplication of discoveries in natural science enormously amplified knowledge of the environment, that the idea of progress was clearly formulated and became generally accepted, and that man became so hopeful of the future.

To-day we have to ask ourselves not whether medicine has progressed but at what rate progress has occurred. The scientist would have great difficulty in finding a formula by which to solve this problem. The only method that suggests itself is that of comparison. Let us, therefore, take a sample period from times past, and for a few minutes consider an imaginary course of Harvey Society Lectures given a hundred years ago, from 1805 to 1830. Who might have been our lecturers and of what progress could they have told us?

This era is not considered by historians an outstanding one as regards medical progress, but we are prepared for some advances, since in other realms of human interest men's minds showed no signs of sterility. Keats and Shelley were making their great additions to English poetry; Beethoven during this period composed all but two of his symphonies, and Goethe wrote "Faust," besides making contributions to comparative anatomy and metamorphosis of plants of no mean importance. Rapid changes were also taking place in men's habits of life. The steam locomotive was being developed, gas was becoming a common illuminant in the houses and streets, thus making transportation more rapid and lengthening men's hours of activity.

In our hypothetical course we should not have had many lecturers dealing with infectious diseases, but we should have had Edward Jenner. The subject of his lecture would, of course, have been vaccination, but he could also have told us something about the reactions (now called allergic) which he had observed in vaccinated persons who had previously had smallpox. Daniel Drake would have been invited to speak on epidemiology, although his classic book on "Diseases of the Interior Valley of North America" was not published until somewhat later. Possibly he would rather have spoken on medical education, since his papers on this subject have been called "the most important contributions ever made to this subject in this country." We should also have asked Elisha North to come down from New London and talk about cerebrospinal meningitis, as his book giving the first description of this serious disease was published in 1811. There were other American physicians and scientists (most of the scientists of those days were physicians) who might have been invited, but then, as now, we should have endeavored to obtain as much foreign talent as possible. Auenbrugger was getting too old to make the long journey, but after the publication of Corvisart's book in 1818 we should certainly have invited him to come over and discuss the new method of percussion. An invitation would also have been sent to Piorry to address us on mediate percussion and to show his pleximeter. Laënnec would, of course, have given us a lecture, and not only demonstrated his stethoscope, but told us about a half dozen chest diseases we had never heard of. Louis would have been one of our best lecturers, for he could have presented abstracts from his masterly book on phthisis or from that on typhoid fever. But, more important, he could have told us much about the new so-called numerical method for studying disease. His method, however, was not very

complicated, the essential features of it consisted in making careful observations and keeping records. He would not have presented very complicated mathematical formulas. Moreover, it would have been interesting to have had a full-time teacher of medicine. He was one of the first. There would have been a very distinguished group of clinicians among the lecturers; Bretonneau would have lectured on diphtheria; John Cheyne would have talked to us about a peculiar type of respiration; Robert Adams, about heart block, although he did not give it that name; Thomas Hodgkins, about a new disease of the glands, and of course Sir Dominic Corrigan, who would have given a lecture on the pulse. One of the best lecturers would have been Richard Bright, who in 1827 published his description of nephritis. Several years earlier we might have had a lecture by William Charles Wells, a native of Charleston, who, in 1811, pointed out the relationship between dropsy and albuminous urine and thus prepared the way for Bright. We should thus have presented to our New York audience the two men who have made the most important observations concerning nephritis from that day to this. A German clinician whom we should have attempted to obtain as lecturer was Schönlein, for he would have addressed us concerning the importance of examinations of the blood and urine, especially chemical examinations, as he was an ardent advocate of this kind of clinical study.

But besides physicians we should then, as now, have invited anatomists and physiologists, chemists and physicists. Among the anatomists, we should have invited Lamarck and Cuvier, and also the German comparative anatomist Johann Friederich Meckel, who, as you know, was also a pathologist.

Unfortunately, John Hunter had died ten years before our course began, and Claude Bernard was not born until 1813, but we should have had Magendie, who would probably have spoken of his experiments concerning digestion. He might also have described his observations regarding sensitization to egg white. Johannes Müller was a little young, but he might have come over at the very end of the course and lectured on "Law of Specific Nerve-energies." We should earlier have had a lecture by Sir Charles Bell on the differentiation of sensory and motor nerves, but Müller would have elaborated and developed this theme. Our own William Beaumont would certainly have been invited to speak on the physiology of gastric digestion, and I imagine he would have been so pleased by the invitation that he would have brought Alexis St. Martin with him for demonstration.

There would have been some chemists, too, of first-rate standing. At the very end of our course we should have invited Liebig, even though he were still quite young. He discovered hippuric acid in 1829, and the year before his associate, Wöhler, had succeeded in synthesizing urea, so we should have made a great effort to get one or both of them. Humphry Davy, or Sir Humphry if he had not come before 1812, would have been one of our most popular lecturers. He would have brought his apparatus and performed experiments before us as he did at the Royal Institution. He would probably have demonstrated the anesthetic effects of nitrous oxide or some member of the audience.

We might have had one or two physicists also, although at that time their work did not seem to have any direct relation to medicine. However, Thomas Young was a doctor and he might have lectured on the differences between the physical and physiological properties of light, or even on the circulation. At the dinner before the lecture he might have told us something about his deciphering the Rosetta stone.

It is true that some of the men I have mentioned might have been overlooked when sending out the invitations to lecture before the Harvey Society. Certain of them were ignored by their associates; others were openly opposed. Some who were most loudly acclaimed in their day are now ranked much lower.

I have described this hypothetical course of Harvey Society Lectures in the years 1805 to 1830 in order to recall to your minds the state of medicine one hundred years ago, and to indicate the kind of men who were making contributions to medicine during these years. It is evident that the chief advances being made were in somewhat different directions than those in which the advances during our own era have occurred. There was great activity in clinical description and in the differentiation of diseases. The center of medical advance was undoubtedly in France, where new methods of clinical investigation, which even to-day are of first importance, were being devised. Physiological discoveries of great significance in pathology, especially as concerns diseases of the nervous system and of the digestive tract, were being made. Finally, advances were being made in chemistry and physics which were of material aid in increasing knowledge about disease. It is evident, however, that comparatively little of this advance originated in America. In our hypothetical course of lectures most of the talent would have had to be imported.

As has always been the case in science, the discoveries of the period are associated with the names of individuals, and as time has passed these men have received an ever-increasing glorification. Neverthe-

less, they must have had great intellectual vigor and possessed high powers of imagination. This is evident not only from the methods they employed in solving their problems but from the actual height of the steps which were mounted. Starting with little knowledge, they scaled great heights with comparative suddenness.

During the seventy-five years which elapsed between our hypothetical course of lectures and the opening of our present course in 1905 important advances were made. During this period occurred the development of experimental physiology, and later the extraordinary growth in pathological anatomy, especially that which resulted from the formulation of the cell theory. Then came the important discoveries regarding infection and immunity, which increased knowledge concerning disease as had never occurred before. The advance in medicine in the last third of the nineteenth century will undoubtedly always be considered to be related to infectious diseases.

In the middle of the century physiology had turned its back on vitalism and maintained the possibility of a physicochemical explanation of all life phenomena, going even so far as to maintain that in the "ultimate analysis biology is only a branch of physics and chemistry."

The great increase in knowledge of the structure of the proteins which took place around the turn of the century led to high hopes that an understanding of these substances would go far in revealing even the nature of life itself. Advances in chemistry were not confined to structural chemistry, however, but a new science developed which had for its parents both physics and chemistry, and had for its content the dynamics of chemical reactions. Shortly before the opening of our era the possibility of direct measurements of energy exchange in man was made possible by the construction of chambers in which men, even sick men, could be studied with the greatest attention to detail.

At the beginning of the era we are considering, therefore, rapid progress in the knowledge of infectious disease was occurring. Progress in organic chemistry was at a high level, and probably this field seemed to offer the greatest hopes for fundamental advances in biology and medicine. There was developing a tendency to lay emphasis upon the importance of studying biology from the dynamic standpoint, "regarding an animal as something that happens."

Germany was at the height of her activity and a greatly increased momentum was observable in this country in the study of the underlying features and phenomena of disease.

Thus was the stage set for the course of lectures designed to promulgate the new knowledge concerning disease as fast as it should be disclosed. It was a happy and fortunate inspiration which in 1905 led Dr. Lusk, Dr. Meltzer and a group of their associates to found this society, at a period when interest in scientific medicine was beginning to glow more bright, not only in New York but throughout this country.

The Harvey Society Lectures do not deal with any single phase of human biological phenomena. They represent a sort of symposium in which workers from various fields of science report their results. In choosing the lecturers, however, the attempt is made to bring together men who have some interest in the problems of human disease, though it is realized that at times this interest may be very remote. In discussing the advances in medicine which the lectures disclose, therefore, one must carefully delimit the field and not include all the results presented. For example, it might be very advantageous for physiologists to have a course of lectures in which physicists, chemists, psychologists, geneticists, anatomists, bacteriologists, even mechanical engineers were asked to speak. They might all contribute new knowledge which would be very important for physiologists to know about, and new facts which might have a very close bearing on physiological problems. Yet one could not assume that all the discoveries in these various fields represented new contributions to physiology. In the past there has been a tendency to assume that all contributions to physiological knowledge or that all advances in biological chemistry represent advances in medicine. Indeed, it has even sometimes been intimated by the votaries of these and certain related sciences that the advances in these sciences form the only contributions to medicine that are of real importance. In my opinion, both physiology and medicine have suffered from this concept.

As has been pointed out, "the various branches of science are not limited by the training and antecedent interests of the persons who cultivate them, but are defined by their subject-matter." Medicine has for its subject-matter disease in its various aspects, and disease involves modification of function, but it also involves modification of structure, whether this be conceived of only in its more superficial aspects, morphology, or its more intricate nature, chemical or physical. But not all modifications of function or structure constitute disease, at least in a practical sense. Although any disturbance of function is probably accompanied by alterations throughout the entire organism, medicine is really concerned with particular, usually gross, alterations in certain specific functions which constitute the symptoms of disease.

Medicine has for its field phenomena which occur in nature, not hypothetical possibilities. The student of disease is interested not only in describing and understanding these disturbances, but in determining the factors, intrinsic or extrinsic, on which they depend. And, just as in the other sciences, even physics, its disciples are interested in obtaining accurate knowledge in order that predictions may be made, and even that the natural course of events may be modified.

The student of disease is interested in all physiological problems for the light that may be thrown on disease processes. The student of physiology is interested in certain problems of disease for the light that may be shed on physiological problems. But he is not interested in all problems of disease, except as matters of general interest. He is not primarily interested in etiology or causation, so far as they relate to external agents, or to environment; he is not keenly interested in the voluntary modification of disease processes, or therapy; he is not deeply interested in the psychological aspects of disease. He is not necessarily interested in disease at all. The interests of the student of physiology and those of the student of medicine overlap, but they are not identical, nor are the contents of these two sciences identical. Virchow was wise enough to see that "each department of medicine must have its own field and must be investigated by itself." As he said, "Pathology can not be constructed by physiologists, therapeutics not by pathological anatomists, medicine not by rationalists," nor, may be added, by chemists, physicists or mathematicians.

If our attention is confined to the results presented before the Harvey Society it will be necessary to omit from consideration certain special fields relating to medicine which have barely been touched upon in these lectures. This is especially true as regards psychiatry and the pathology of the nervous system. Such important developments as conditioned reflexes, the study of behavior, the newer modes of thought concerning psychoanalysis and psychotherapy have been considered very briefly if at all. So too in these lectures comparatively little attention has been given to the great advances which have been made in surgery, not only as regards the technique of operating and maintaining asepsis, but also as concerns the improvement in methods of diagnosis and treatment of so-called surgical conditions, advances which are based on recent discoveries in physiology.

We shall also have to omit from consideration certain great movements, such as the organization of private and governmental agencies, and of the medical profession, whereby applications of new knowledge concerning disease can be made rapidly and to a previously unbelievable extent. This has certainly been an outstanding feature of the present quarter century.

Medical education has undergone an extraordinary extension, and a very striking modification in method, especially as concerns organization and teaching, has occurred in the medical clinics during this period. Whatever the effect these changes may have have had on the education of students, and thus on practice, they have resulted in a tremendous increase in the opportunities for the investigation of disease. These opportunities consist not only in better material equipment in the way of laboratories, but also in protection of the followers of the science of medicine from the burdens of private practice.

These are all matters which have been very lightly touched upon in the Harvey Society Lectures, but they can not be neglected when thinking of the history of medicine during these twenty-five years, as it will be written by our followers.

To point out certain specific outstanding contributions to medicine is not difficult. Knowledge concerning several important diseases has been enormously increased.

One of these diseases is syphilis. At the time the course of lectures began the nature of the inciting infectious agent was unknown and diagnosis depended entirely on superficial clinical features. The relationship of tabes and general paralysis to this infection, though strongly suspected, was uncertain. Its treatment was fundamentally that of a hundred years before. During the period, the inciting agent has been isolated, even cultivated, and in most instances may be demonstrated in the lesions; a reliable, accurate, purely objective, quantitative method of diagnosis has been devised; the specific nature of tabes, general paralysis and of many other manifestations of the infection, such as aortitis, has been established, and finally a greatly improved method of treatment has been devised. Moreover, the disease has been produced experimentally in animals, and very much knowledge concerning the mode of infection and the reactivity of the host, as exhibited by hypersensitiveness and immunity, has been obtained. Hardly in the whole history of medicine has such a striking increase in knowledge concerning any important disease occurred within so short a period as twenty-five years.

Also a great increase in knowledge has occurred concerning certain forms of heart disease. Shortly before the beginning of our course of lectures anatomical studies had demonstrated the presence in the heart of special fibers having the particular function of conducting the impulses giving rise to contraction. Through the intensive study of arrhythmia in pa-

tients, at first by very simple instruments, even by direct observation and palpation of the arterial and venous pulse, and later with the aid of a galvanometer especially suited to the study of these problems, it has been possible accurately to localize the specific lesions upon which the various types of arrhythmia depend. Knowledge has also been gained concerning the effects of certain drugs in modifying rhythm, and as a result it has been possible to employ these drugs with greatly increased accuracy and efficiency.

At the beginning of the era knowledge concerning diabetes was fragmentary. Much was guessed but little was known. During the past twenty-five years many facts concerning the metabolism of sugar in health and in disease have been disclosed, the underlying factors in the production of coma have been determined, the disease has been accurately reproduced in animals, the demonstration has been made that a substance secreted by the pancreas greatly influences sugar metabolism and that the disease is associated with the lack of this substance and, finally, a practical method of supplying this substance, when lacking, has been devised, so that the symptoms of the disease may be made to disappear.

The more recent contributions to knowledge concerning pernicious anemia are also significant. This most serious malady has remained one of the mysteries of medicine ever since its description by Addison in 1849. Now, by a series of experimental studies, not only has a practical therapeutic measure been found, but it seems not unlikely that much progress has been made toward understanding its essential nature. The culminating discovery that in this disease the production of new red blood cells may be stimulated by the intravenous injection of a few drops of a solution of a substance normally present in liver, and to a less extent in other tissues, signalizes a notable triumph for the experimental method.

The discovery that in rickets the phosphorus as well as the calcium metabolism is disturbed, the demonstration of the therapeutic value of sunlight in this disease and especially the demonstration of the remarkable fact that anti-rachitic properties may be conferred upon particular fatty substances by exposing them to ultra-violet light rays of definite wavelengths, and that the specific reaction which is thus induced consists in a polymerization of ergosterol, seem to me to be of extraordinary theoretical interest as well as of practical value.

These are a few of the diseases concerning which striking and significant new knowledge has been obtained. They have been specifically mentioned because in these instances, as a result of new knowledge, improved methods of treatment have been developed. In many other instances, however, although no practical results have so far been obtained, much has been learned about particular pathological phenomena.

In all these cases various sciences have contributed to the advances, although it is impossible to evaluate the relative importance of the rôle which each of them has played. While in most instances the discoveries did not depend upon the most recent advances in physics and chemistry, it is certain that they could not have been made in the absence of the organized systematized knowledge which comprises natural science. Nor could they have been made without the growth in anatomical and physiological knowledge which has occurred during the past three hundred years. The facts of importance to medicine, however, did not emerge spontaneously from the accumulated knowledge of the past. In most instances the discoveries were made because some one was interested in the problems of the particular disease, and because some one thought of a new way of solving these problems, using of course for this purpose any of the accumulated knowledge, or any technique of any science, that was suitable for his purpose. This is not only the prerogative and custom of the followers of the science of medicine but it is the method employed in every other science, including that of physics.

In certain of the instances which I have mentioned, the discoveries were not the outcome of entirely new modes of thought or procedure. The emergence of these discoveries can be traced to specific preceding discoveries which supplied the example or pattern to be followed. For example, in several instances the discoveries have to do with so-called internal secretions of the ductless glands, or with a deficiency of these secretions. In the middle of the last century clinicians observed that, in individuals who exhibited special groups of symptoms, pathological lesions were present in certain glands. This was a discovery of great significance which physiology owes to medicine. It was found that in certain instances removal of these glands from animals was followed by symptoms similar to those seen in patients in whom the same glands were affected. Gradually evidence accumulated which indicated that in some cases the function of the diseased glands could be replaced, at least in part, by feeding the fresh glands of normal animals, by grafting, or better, by injecting extracts of these glands. The conception, however, that these glands secrete chemical substances, or "messengers," by means of which "correlation of the functions of the organism are brought about through the blood, side by side with that which is the function of the nervous system" is a physiological principle well established

only in the present era, and one which is probably of great significance both to physiology and to medicine, and may possibly prove to be the most important contribution made to medicine in the present era. The fact that at least two of these "messengers," or hormones, have been isolated, and their chemical constitution established by American workers, exemplifies in a striking manner the interdependence and helpfulness of the various sciences, and also indicates the important position which American investigators have come to occupy.

Another example of chemical coordination through the blood was given by the discovery that the respiration is regulated by the carbon dioxide tension of the arterial blood, or more properly, by the H^+ ion concentration of the arterial blood, acting on the respiratory center. The physiologic importance of the maintenance of the neutrality of the blood which was thus emphasized has led to very extensive and accurate investigations of the mechanisms involved in maintaining the "constancy of the internal environment," a happy phrase coined long ago by Claude Bernard. This work is undoubtedly of much importance, especially for physiology but also for medicine. But I should again like to emphasize that not all disturbances in equilibrium constitute disease. It is only when these disturbances exceed the limits of the factors of safety, as described by Dr. Meltzer, that disease may be said to occur.

Another field of physiology in which great activity has taken place during the present era is that of total metabolism or energy exchange in the body, and this is reflected in the considerable number of lectures dealing with this topic. It is to the great credit of American workers that much knowledge has been gained concerning metabolism under pathological conditions.

Also in the field of nutrition, the discovery has been made that not all proteins are capable of supporting life, but that proteins containing certain specific amino acids are essential. The great advance in the field of nutrition, however, was made by the demonstration that animals can not live and thrive on a diet composed solely of pure protein, fat and carbohydrates combined with inorganic salts and water. Certain other "accessory food factors" were shown to be necessary. When these are lacking, disease supervenes, and this fact has been of value in explaining certain diseases, now called deficiency diseases, such as beriberi, rickets and probably pellagra. Certain analogies have been pointed out between the vitamins and the hormones, indeed the former have been called exogenous hormones. The chemistry of the vitamins and the nature of their action, however, still remain to be studied thoroughly.

Another advance in physiology which is of great significance for medicine consists in the demonstration of the rôle which so-called oxygen carriers play in oxidations within the body, and the demonstration of reactive, ferment-like substances which stimulate oxidation.

In the study of infectious agents and the reactions of the body to parasitic invasion, progress has also been made in many directions. Many of the results obtained, however, have undoubtedly consisted in the application and extension of discoveries which were made during the latter decades of the last century. The important relation of the so-called filterable viruses to human diseases has been demonstrated, and the evidence suggests that this importance is even much greater than is now obvious. The conception of "haptens" and the investigation of the chemical structure of the bacteria, especially in relation to their antigenic properties, the introduction of specific local therapy are all directions of activity which afford promise of wide application. Whether, however, advances in the field of infectious diseases have taken place at the same rate as in the preceding era seems doubtful.

Time will not permit me to speak of the specific contributions of organic chemistry to medicine during this era. Much attention has been given to the constitution of the chemical substances isolated from the tissues and secretions; many more than 200,000 organic substances (mostly synthetic) have now been analyzed and investigated, and much study has also been given to the intermediate stages through which organic compounds pass in their transformation within the animal body. A particular development in this field, namely chemotherapy, has possibly not entirely fulfilled the expectations that were aroused by its great success in supplying a remedy in the treatment of syphilis. Nevertheless, the introduction of this essentially new mode of thought and procedure is of great significance, and it occurred in our era.

Not only have the new developments in physics, especially in the field of light and of electricity, received wide application in the study of biological phenomena, but a new branch of physics, biophysics, has developed. The use of X-rays in diagnosis has been greatly extended. More recently the study of the physiological effects of X-rays and of light of various wave-lengths is being made.

It is obvious that I have been able merely to mention a few of the topics discussed in the Harvey Society lectures. The professional activities of the 220 lecturers indicate to some extent the fields covered. It is rather surprising to find that the largest group of lecturers consisted of clinicians, of whom there were fifty-two; the next largest

group was composed of physiologists; the other groups, arranged in order according to size, consisted of biochemists, bacteriologists and parasitologists, pathologists, biologists and geneticists, anatomists and pharmacologists. The list of lecturers has included many of the most distinguished students of medicine; about one fourth of them were from foreign countries.

As one goes over the twenty-four volumes containing the Harvey Society Lectures (the omission of one volume represents one of the losses of war) he can not help experiencing a sense of mystery, almost of awe. Here, beside the wealth which is very evident, there also undoubtedly lie hidden masses of gold, which in many cases are unsuspected, even by the donors. In future years some one will discover and make use of them and reveal riches to us of which we can not dream. On the other hand, these volumes probably conceal deep tragedies. Instead of leaving to their scientific descendants what they believe to be fabulous treasures, some investigators have probably left only ashes to be scattered and lost.

That the number of workers in the science of medicine has tremendously increased during this period and that there is no lack of activity are shown by the wide expansion of the medical literature. In his presidential address before the Thirteenth International Physiological Congress, Professor Krogh stated that in the first year of the century titles were given in the *Zentralblatt für Physiologie* of 3,800 papers; in 1926 there were 18,000. Moreover, that, while in 1901 there were only one hundred papers, or 2½ per cent. of the total, published in America or by American authors, in 1926 there were 3,500 papers, or nearly 20 per cent., from this source. What has occurred in physiology has taken place also in medicine. Professor Krogh also had the temerity to state that in his opinion "too many experiments and observations are being made and published and too little thought bestowed upon them."

During the past twenty-five years there has been a gradual change in the kind of investigation employed in the study of disease and in the methods used. It is only a comparatively few years since Rokitansky expressed the conviction "that pathologic anatomy must be the foundation not only of medical knowledge but also of medical treatment, yes, that it contains everything that there is in medicine of positive knowledge and of foundations for such knowledge." It is evident, however, that during the present century interest in the so-called descriptive sciences, such as anatomy, morphological pathology and possibly organic chemistry, has waned. Indeed, most of the anatomists who have lectured before the Harvey Society have not discussed structure at all. With the anatomists and pathologists experimentation is replacing observation. At the beginning of the century high hopes were entertained for the results that were to follow the chemical analysis of the cells of the body. One of the lecturers has stated that "the action of the cell depends essentially on the nature and quantity of the various substances of which it is made." The same complaint, however, that had been raised against pathological anatomy, namely, that it is concerned only with dead material, began to be raised against organic chemistry. Even the chemists themselves suggest this. One of the most distinguished said in a Harvey Society Lecture, "these descriptive studies [meaning structural chemistry] we may regard as a sort of chemical anatomy of the human body." The biochemists are also becoming experimentalists, employing the methods of chemistry only more or less incidently.

Careful observation and description are no longer fashionable. Even the word "description" causes a certain shrinking, or a shrugging of the shoulders, depending upon who utters it. At the very beginning of the century there occurred a marked tendency to return to the methods of experimental physiology, the kind of activity developed by Magendie and Claude Bernard. But reflections are now being cast even on this kind of investigation. It has been maintained that the entrance of bacteriology on the stage, in the last quarter of the nineteenth century, for a time displaced physiological experimentation. One writer said a few years ago, "With Pasteur and his successors the will was more important than the reflective intellect, and this interlude [the bacteriological] had the effect of narrowing the outlook and rendering medicine less rational." And again, "In default of the physicochemical foundations, during a period when bacteriology was the dominant influence in medical science, and next to it, perhaps, the highly specialized science of organic chemistry, when the prevailing activity was somewhat unintellectual, physiology continued along the old paths."

To my mind this attitude toward bacteriology seems narrow and unjustifiable. However this may be, there is little doubt that during the present century the influences which we have previously noted, especially the attempts to obtain a physicochemical explanation of life itself, and the promulgation of the idea that "physiology is but a special case of the physics of the colloids and the chemistry of the proteids" have led to a very distinct and striking shift in the thought and methods of physiology which is also affecting medicine. To designate this new physiology the term "general physiology" has been employed, or it has been called abstract as contrasted with applied. The field of general physiology, how-

ever, does not seem to be very accurately defined, and sometimes the term is used to limit the field to the study of phenomena which are common to all living matter, and again is used to indicate the methods employed in investigation. It may be said, however, that the main problem of general physiology is to describe the properties of living matter in purely physicochemical terms.

All these problems of methodology, however, do not concern students of disease except indirectly. Medicine is indeed a part of biology, but it is only a part. Through the study of disease broad biological generalizations may emerge, as they have in the past. But the immediate problems of the student of disease are not the problems of the biological philosophers or even of the physiologists. The student of disease is trying to describe and to understand the interrelationships of certain special phenomena with which he comes in contact. Even Galileo was content to ask how, not why. In recent years there has seemed at times to be some uncertainty in the minds of those professing the investigation of disease as to exactly what they are studying, possibly a reflection of the confusion in the ranks of the physiologists. It seems to me, as it has seemed to many others, that at least one essential in investigation is that there should be a question asked. If the question relates to disease, then the person who tries to answer it becomes a student of disease, whether he be clinician, physiologist or anatomist. On the other hand, and this is important as regards future advance in medicine, a man is not necessarily a student of disease because he is a doctor of medicine or because he works in a laboratory of medicine, even though he may contribute ever so greatly to science, as, for instance, did Gilbert or Young or Mayer, or be as important in philosophy as was John Locke. Questions concerning disease will most frequently arise in the minds of those coming in contact with disease, though they may arise in the mind of any intelligent person. It seems, however, that the person who most carefully observes and describes the phenomena of disease will ask the "best" questions. The method employed to answer the question or to solve the problem will then have to depend upon a decision as to which method is most appropriate. Whether or not the observer can attempt the solution will finally depend upon whether or not he possesses a sufficient mastery of the appropriate technique to justify his undertaking the task.

In attempting to answer biological questions it seems to be generally conceded that the method which has been found most rewarding is that of hypothesis and test, or as it is called, experimentation. Now in performing an experiment, accurate and careful observation and description are just as important as they are in formulating the question. One wonders, therefore, whether there is not an inherent danger underlying the present tendency to scorn and belittle observation, and whether the possibilities of clinical medicine, and anatomy and morphological pathology, and organic chemistry were all exhausted in the nineteenth century. The experience of the past twenty-five years seems to indicate that this kind of investigation still brings its rewards.

In description, various kinds of yardsticks may be employed. For describing some phenomena extremely accurate quantitative measurements, even formal mathematical treatment of the results, in order to reveal hidden quantitative relationships, are appropriate. For describing other phenomena such measurements are not only unnecessary but quite unsuitable. In recent years there has been a tendency to assume that great accuracy in measurement and the use of higher mathematics in the study of the problems of physiology and disease at once endow the investigation with a sacerdotal dignity. This is also true of the use of the methods of chemistry, physics and physical chemistry. One of the great advances made in the present century consists in the fact that now many students of medicine are trained in these sciences and have more or less mastery of their techniques. But discrimination is necessary in their employment when attempting to describe disease processes. The student of disease should be certain that he is trying to learn about disease and not merely exercising his technical skill. One needs only to recall some of the absurdities and futilities of the iatro-mathematical and iatro-physical and iatro-chemical schools of the seventeenth century to realize the dangers inherent in this attitude of mind. Sanctorius is said to have spent forty years of his life in weighing himself three or four times a day.

Furthermore, there has grown up a certain sanctity about the word experimentation which seems to me to be unjustifiable. Experiments are of two kinds: first, the true experiment carried out to test a hypothesis; and second, the more or less random procedure undertaken to see what may happen. These latter experiments, made without hypothesis, can have only one purpose, and that is, to afford opportunity for observation. As Claude Bernard pointed out, such experiments are at times valuable since, in making the observations, hypotheses are suggested, and these can then be verified or disproved by true experimentation. But the student of medicine has little need for such groping for material. He is daily surrounded by phenomena which are stimulating beyond measure if he but have eyes to see.

It has been assumed that during the present era medicine has become more rational. The introduction of rationalization into medicine is of extreme importance, just as is its employment in all scientific activities. John Hunter's advice, "Don't think, try," is all very well in the meaning intended, but the injunction must not be taken too literally. Think first, then try, may be a better maxim. And on what one thinks about will depend what he will do. But the question arises whether the present trend in medical investigation really fosters thinking. Modern medical education has supplied an army of trained technicians. Are they all asking questions concerning disease and attempting to solve them, or are many of them only interested in desultory and fragmentary employment of the techniques they have acquired, having faith in the Baconian concept, that if a sufficient number of observations and experiments are made, the connections will appear and general truths automatically evolve? Such an attitude of mind seems to belong in the seventeenth century, not the twentieth.

What I have said does not mean that the student of disease must always be attempting a direct approach to the solution of his problem. Usually it is necessary to start far away from this goal and often to take a circuitous path, but he should always have the goal in mind, otherwise he really belongs in some other field of scientific endeavor. It has recently been said that "for the first time mathematics, physics, chemistry and physical chemistry, as aids to physiology, have passed into the hospital." I can not but feel that the phrase "as aids to physiology" was introduced by the writer inadvertently. But it is possibly true and may be of some significance.

One wonders whether if the student of disease did but observe, and then describe in language appropriate to the phenomena observed, following Daniel Drake's advice "to write much and publish little," and then if he would think, and think until it hurts, and make experiments only when he has evolved a hypothesis that interests and satisfies him, performing a sufficient number of experiments and employing a technique appropriate for the particular purpose, but publishing only when he had satisfied himself that a conclusion had been reached, even if negative, not only might the bewildering number of publications be reduced, but the increase in knowledge be materially accelerated. For as Professor Whitehead says, "The growth of a science is not in bulk but in ideas." Perhaps this is heretical doctrine, and no one realizes its dangers better than I. During the past twenty-five years it has been important, at least in this country, that young men be stimulated to investigate. And nothing so urges a beginner to further effort as to witness the birth of his labors. Moreover, there is nothing so much feared at present as inactivity. But is it not time for this naïve attitude to be dropped?

May there not be a lesson for us in the history of physics during the present era? A recent history of science states that "at the end of the last century, it seemed that all that remained for the physicist to do was to make measurements to an increasing order of accuracy." It goes on to describe how physics then suddenly took on new life. New concepts were born. The atom was resolved into more minute corpuscles and these in turn into electrical units. The old concept of mass was overthrown and a new one took its place. Radioactivity was interpreted in terms of atomic disintegration. The quantum theory of radiation superseded the wave theory, or at least was added to it. Space and time became no longer absolute. A particle became a mere series of events in space-time. Physicists have become less certain than they were at the beginning of the century.

Biology and physiology and medicine too have come to have some misgivings, but so far these doubts have not been very coherent or articulate. The speculations of men like Whitehead, who emphasize the relation of the organism to the environment, the development of the theory of emergent evolution, which Jennings calls "the Declaration of Independence for the biologist," the concept of biology as an independent science by Haldane and his followers, have all exerted an influence in stimulating the study of the organism as a whole and not merely as an agglomeration of parts. Nevertheless, while in the study of disease it is not necessary finally to accept any theory of the ultimate nature of life, it is difficult to conceive of any successful method of procedure which in all its steps does not assume a physicochemical basis for living things. This does not mean, however, that it is necessary to make graven images of chemistry and physics. At any rate, the question may be raised whether in the study of disease it is always necessary to resolve the organism into electrons, or whether advances can not be made also by studying the organism itself. Certainly the history of the past twenty-five years, as of all preceding periods within the era of modern science, seems to answer this question in the affirmative.

Looking backward, one wonders whether it would have been possible for any one to foretell the directions in which the greatest progress would be made in medicine during the quarter century just passed. Probably the greatest promise seemed to lie in fields other than those which have apparently yielded the

most important results. It would therefore be hazardous to attempt to predict the future. But of one thing we may be sure, the foundations on which the future is to be built have been rendered more solid, more substantial; the builders who are to undertake the new tasks are enormously increased in number; they are better equipped; they have a wider knowledge of the fundamental sciences; they have acquired greater technical skill in experimentation; they have at their disposal greatly increased facilities. This insures a continuation of progress. There is some evidence too that the workers are trained to think more logically and rationally than their predecessors.

But after all, probably what is needed most in medicine is not method but men, and not merely photographers but artists. Whether the coming era will be a golden age depends on whether in medicine "there shall be minds acting upon thoughts so as to color them with their own light, and composing from these thoughts, as from elements, other thoughts, each containing within itself the principle of its own integrity." For these geniuses we are dependent upon the gods.

THE HARVEY SOCIETY[1]

By Professor GRAHAM LUSK

CORNELL UNIVERSITY MEDICAL COLLEGE

President Hartwell, of the Academy of Medicine,
President Robinson, of the Harvey Society,
Ladies and Gentlemen:

THE story of the birth of the Harvey Society is a simple one. I was dining in the old Lusk home at 47 East 34 Street and sat next to Mrs. Anna Bowman Dodd. You will remember that it was she who wrote many years ago "Three Normandy Inns." The greater part of her life she lived in France; in Paris in the winter, and in a beautiful home at Honfleur on the Normandy coast in the summer. She has recently passed away at the age of about eighty. It gave her pleasure to the end to be told that she was the real founder of the Harvey Society. At the dinner to which I refer she said that during the winter she had attended a course of splendid lectures at the Sorbonne upon the subject of Roman law expounded by a brilliant Frenchman. It occurred to me that if an educated American woman past middle life could be thrilled by lectures on Roman law, there must be physicians in New York who would be interested in hearing lectures on scientific subjects as expounded by scientific workers themselves. There was only one man with whom to go into conference on this subject and that was Dr. Samuel J. Meltzer. Meltzer had already used the library of my home at 11 (now 9 and rebuilt) East 74 Street, for in it a few years before, he had founded the Society for Experimental Biology and Medicine, sometimes for the sake of abbreviation affectionately known as "The Meltzer Verein." This was to be a society of scientific *workers*, and is to-day a notable feature of the Academy of Medicine. In response to a telephone call Meltzer came to see me immediately and, sitting together on a sofa, I outlined my plan. He said the idea was impossible; New York was a city devoid of scientific interests. The Academy of Medicine was not a scientific body and had no interest in scientific medicine. No one would come to the meetings and it would be futile to start such a movement.

A few days after this Meltzer called me on the telephone and said, "You must call that meeting at your home." I replied, "But, Dr. Meltzer, you said the plan was impossible." "Ah, but I have changed my mind."

So it came about that there met at my home on the anniversary of Harvey's birth, April 1, 1905, the following group of men: Meltzer, W. H. Park, E. K. Dunham, Ewing, Lee, Herter, Flexner, Wallace, T. C. Janeway, Levene, Opie, Abel, of Baltimore, and Lusk. I outlined the plan. Every one objected, using the same arguments which Meltzer had originally used against it and which Meltzer now convincingly answered. His final words were, "Never mind if no one comes except ourselves. We will wear our dress clothes, sit in the front row and show the speaker that we appreciate him."

We drafted as simple a constitution as possible. The society was described as founded for the diffusion of knowledge of the medical sciences. The active members were to be laboratory workers who were to choose a president and other officers annually. The lectures were to be on scientific subjects by masters who had worked upon the themes they presented. The associate members were to be practicing physicians who represented the best types in the city. This list was originally selected by Meltzer, Dana, then president of the academy, and by Abraham Jacobi. Meltzer remarked, " I wish to have this list so select that when a man comes to die it shall be said of him, 'He was a member of the Harvey Society.' " Scarcely any one who was invited declined.

[1] Address delivered at the twenty-fifth anniversary of the Harvey Society, May 15, 1930.

The Society of Biological Chemists, the soul of which was undergoing transmigration into the body of the Meltzer Verein, bequeathed to the Harvey Society $100, the total sum of cash in its treasury. This was the first and last donation ever given to the society.

Originally the lecturers received no fee, but their traveling expenses were defrayed and they were entertained while in New York.

The Harvey Society seemed an especially appropriate name, since Harvey, among other great contributions to the Royal College of Physicians in London, had established an annual oration in which the benefactors of the college were to be commemorated and the fellows and members of the college were to be exhorted to search out and study the secrets of nature by way of experiment and to continue in mutual love and affection among themselves.

The first lecture of the Harvey Society was given in German by Professor Hans Horst Meyer, of Vienna. At this lecture the society was formally presented to the public by Dr. C. A. Dana, then president of the Academy of Medicine, and he declared it to be under the patronage of the academy. The society owes much to Dr. Dana. The second lecture was given by Dr. Carl von Noorden, and there was standing room only in the hall. The anxious concern over the success or failure of the undertaking came to an end.

The society was made up of a group of young men. I remember giving a dinner of thirty to Professor Max Rubner, of Berlin, nearly twenty years ago, and he, surveying the table, said to me, "You have no old men in America." As far as our scientific group was concerned, this was then true. Scientific medicine in New York stood at the beginning of time.

We sought to develop a forum where young workers in experimental medicine could unfold their ideas for the benefit of the medical profession. The influence of the society was not confined to New York City. Thus, when Woodyatt, of Chicago, was invited to give a lecture before the Harvey Society, he told me that his associates in Chicago began to say to each other that he must amount to something if a group of scientific men in New York thought his work to be of such significance that they asked him to address them.

The solemnity of the society was once disturbed on the occasion of a lecture given on February 15, 1913, by Theodore Janeway on the subject of "Nephritic Hypertension: Clinical and Experimental Studies." A few days before the lecture some of the members of the society received the following postal card printed in exactly the same form as the regular notices:

THE HARVEY SOCIETY

A Society for the Diffusion of
Somnolence by Medical Scientists

The Seventh Lecture will be delivered on Saturday, February 15th at 8:30 p.m., at the New York Academy of Medicine, by Professor Theodore C. Janeway, of Columbia University.

Subject:
"HYPNOTIC HYPERATTENTION: CYNICAL AND EXPERIMENTAL STUDIES"

Professor Janeway was one of the most successful cures made by the society. Testimonials from Dr. W. C. Lusk and others furnished on request.

1 mile 1	1 hour 1
of dazzling charts	without an open eye

Sufferers from Insomnia come!
Let us cure you.

Meltzer came to see me in hot haste. Some enemy of Janeway had done this, who should be apprehended and punished. It developed that Janeway, who was fond of a good joke, had done it himself.

Dr. Cole has told you of the developments of science during the past twenty-five years. The development of science during the next twenty-five years is entrusted to many who are here to-night. Those of us who have worked in the past transmit this heritage to those of you who are to carry on in the future the ideals represented by the Harvey Society. We do this with the firm conviction that during the next twenty-five years scientific medicine will advance as rapidly as it has done in the past quarter of a century.

We are groping in the dark to find the secret of the production of scientific men. They are certainly not produced by the administration of any known patent medicine. Lavoisier, Gay-Lussac, Liebig, Johannes Müller and Pasteur were not developed according to formulas. Ostwald, in his "Grosse Männer," says that the facilities granted to Liebig at Giessen gave him water to swim in. Some of our good scientific men have not been given water enough even to float. Others have been given so much that they have been drowned. The great problem of today is to seek out brilliant young men and establish them so that they may accomplish good work, and may also feel free from financial worries which so often beset them. Such a policy will tend to produce distinguished scientific men in greater number and

will give a greater choice of Harvey Society lecturers than ever before.

I rejoice to sit near my life-long friend and dear colleague, Dr. Hartwell, to share an honorable place with the president and the ex-presidents of the Harvey Society, who have done so much to promote medical science in New York City, and to feel that Dr. Welch and Dr. Cushing have generously given their distinguished presence at this birthday party of the Harvey Society.

SCIENTIFIC EVENTS

NATIONAL MONUMENTS IN ARIZONA

President Hoover has signed the act of Congress authorizing the exchange of privately owned lands in the Petrified Forest National Monument for government-owned lands outside the reservation in Navajo and Apache Counties, Arizona.

The Petrified Forest National Monument contains a total acreage of 25,908.4 acres. Of this 12,792.74 acres are in private ownership, representing original railroad land grants and occupying alternate sections throughout the monument. Such a situation precluded effective administration and also made impossible the construction of an adequate road and trail system to make available to the visiting public the principal features of the monument, since the roads and trails would unavoidably have to pass fifty per cent. over private lands and expenditure of government funds could not be authorized under these conditions. It is to obviate these difficulties that the exchange of lands as outlined above was authorized.

The New Mexico and Arizona Land Company, present owner of the alternate sections, has signified its willingness to make the desired exchange which is solely in the interest of government administration of the monument.

The trees of the fossil forest are not standing, but lie scattered over the ground in great profusion. They did not grow where they lie, but were carried from a long distance to this region by flood waters, became waterlogged and finally sank to the bottom of the great inland sea which once covered the region. Here they lay for countless ages, slowly being covered with silt and sand, and gradually becoming fossilized. Thousands, perhaps millions, of years later the submerged logs, now stone, were through some upheaval brought to the surface again and uncovered. It is estimated that the trees were green and growing about 20,000,000 years ago.

The petrified trees of this area are more highly colored than in any similar area, and there are more of them.

Last year more than 69,000 people visited the Petrified Forest National Monument.

The new Sunset Crater National Monument is located within the Coconino National Forest in Arizona, and will be administered by the Forest Service. The area set aside for the monument totals 3,040 acres, and includes Sunset Mountain with its extinct crater and the ice caves at the foot. These have been points of interest visited by many people each season for the last 20 years.

THE DUDLEY HERBARIUM

The Dudley Herbarium has begun a botanical survey of Lower California. This work is under the direct charge of Dr. Ira L. Wiggins, and has been made possible through the generosity of Mr. H. C. Dudley, of Duluth, Minnesota, and Mr. E. G. Dudley, of Exeter, California.

Professor Wiggins has made two collecting trips into Lower California during the past nine months. The first extended from September 1 to 20, 1929, the itinerary leading through the northernmost part of the peninsula. The route followed the coast from Tia Juana to Ensenada, circled eastward through the Valle San Rafael, crossed the southern end of the Mesa del Pinal and reentered the United States at Mexicali. A more extended expedition occupied the latter part of February and the month of March of this year. During this time field observations and collections were made from the border southward to the desert region in the vicinity of Chapala, about 350 miles from Tia Juana. The winter had been very dry so collecting was rather poor, but a fair amount of interesting material was obtained. Extensive field notes on the distribution and habitat of several interesting species endemic to the central part of the Lower California peninsula were taken and numerous photographs made. An account of this phase of the work will be published later.

A program to cover a period of several years calls for further extensive field work and collecting throughout the entire peninsula and will ultimately lead to the publication of a comprehensive floristic study of the area. Such trips are to be made at various seasons of the year and to little known or unexplored areas in order to fill gaps in the collections of earlier workers.

NATIONAL RESEARCH FELLOWSHIPS IN THE BIOLOGICAL SCIENCES

The second and final meeting of the Board of National Research Fellowships in the Biological Sciences for the award of 1930–31 appointments was held in Washington on May 1 and 2. In addition to the ten

reappointments and twenty-four new appointments voted at the February meeting, ten further reappointments and twenty-two first appointments were made at the meeting in May, as follows:

REAPPOINTMENTS

For domestic study

Norval Burk—biochemistry
C. R. Burnham—agriculture
F. M. Carpenter—zoology
Leonard B. Clark—zoology
Myron Gordon—zoology
Harry Grundfest—zoology
David A. Kribs—botany
Norman R. F. Maier—psychology

For study abroad

Lester G. Barth—zoology
Philip R. White—botany

NEW APPOINTMENTS

For domestic study

W. M. Banfield—agriculture
Ralph L. Beals—anthropology
P. J. Daughenbaugh—biochemistry
Robert K. Enders—zoology
Dwight L. Espe—agriculture
Paul E. Fields—psychology
J. W. Gillespie—botany
J. P. Greenstein—biochemistry
E. Harold Hinman—zoology
A. W. Kozelka—zoology
Chester E. Leese—zoology
Roger B. Loucks—psychology
Helen Mar Miller—zoology
Harold P. Morris—agriculture
Elsa R. Orent—biochemistry
Hortense Powdermaker—anthropology
K. C. Pratt—psychology
Alexander F. Skutch—botany
Otis C. Trimble—psychology
C. W. Watson—agriculture
G. R. Wendt—psychology
Samuel Yochelson—psychology

For study abroad

L. M. Bertholf—agriculture
David B. Hand—biochemistry
J. I. Hardy—agriculture
Ancel B. Keys—zoology
George Kreezer—psychology
Floyd L. Ruch—psychology

In accordance with the plan of rotation in membership of the board the terms of the following members expire on June 30 of this year: T. H. Morgan, zoology; W. J. V. Osterhout, botany, and C. E. Seashore, psychology. To complete the membership in these three fields, the National Research Council has appointed H. S. Jennings, of the Johns Hopkins University, for zoology; Dr. Harry M. Johnson, of the Mellon Institute for Industrial Research, Pittsburgh, for psychology, and Dr. W. J. Robbins, of the University of Missouri, for botany. Also, in order that anthropology may have two representatives on the board, Dr. Fay-Cooper Cole, of the University of Chicago, has been asked to serve for the fiscal year, 1930–31.

Meetings for 1931–32 awards will be held approximately early in February and either in April or May of next year. More definite announcement concerning these will be made in SCIENCE in the fall. Information and application forms may be obtained at any time from the secretary, Board of National Research Fellowships in the Biological Sciences, National Research Council, Washington, D. C.

FRANK R. LILLIE, *Chairman*
BOARD OF NATIONAL RESEARCH FELLOWSHIPS IN THE BIOLOGICAL SCIENCES

THE ANNUAL MEETING OF THE ROYAL SOCIETY OF CANADA

ON May 20, 21 and 22 the Canadian Royal Society met at McGill University. The presidential address was given by Professor A. S. Eve, on the evening of the twentieth, his subject being "The Universe as a Whole." (SCIENCE, May 23, 1930.) At the same session he presented the three gold medals of the society. The Flavelle Medal was awarded to Dr. A. B. Macallum, F.R.S., emeritus professor of biochemistry at McGill University, for his pioneer researches in micro-biochemistry; the Lorne Pierce Medal for outstanding contributions to literature, to Sir Andrew Macphail, professor of the history of medicine at McGill University, and the Tyrrell Medal for research in Canadian history to Dr. Adam Shortt, of Ottawa. At the final session the Honorable Vincent Massey, Canadian minister to the United States, gave the popular lecture on "Art and Nationality in Canada." In Sections I and II (respectively, French and English Literature and History) thirty-six papers were read. In Section III (Mathematics, Physics and Chemistry) ninety-nine papers were communicated, in Section IV (Geological Sciences) 13 papers, and in Section V (Biological and Medical Sciences) 59 papers.

A very interesting event was the delivering of an address by radio by Sir Ernest Rutherford, president of the Royal Society of London, speaking from his English home to the fellows gathered in the Moyse Hall of McGill University. The whole of the address, with the exception of the few opening sentences, was clearly heard by a large audience, as was also the

telephone conversation that followed, between Sir Ernest at the one end, and Professor Eve, Sir Arthur Currie and Sir Robert Falconer in turn at the other. Only ten years previously it had been considered a remarkable event when, also under the direction of Professor Eve, an artist sang at Montreal to an audience of the Royal Society at Ottawa, 110 miles away, and the reception over this short distance was much less perfect.

BRITISH HONORS

FROM the list of honors conferred on King George's birthday, *Nature* selects the following names of scientific workers and others associated with scientific activities: *Baron:* The Right Hon. Noel Edward Buxton, minister of agriculture and fisheries. *Baronets:* Mr. Basil Mott, past president of the Institution of Civil Engineers, and Mr. F. H. Royce, founder, director and chief engineer of Rolls Royce, Limited. *Order of Merit:* Professor S. Alexander, in recognition of his eminent position as a British philosopher and for his services as a writer and teacher. *Knights:* Dr. E. Brown, secretary of the National Poultry Council of England and Wales; Major T. H. Crozier, chief inspector of explosives, Home Office; Professor A. S. Eddington, Plumian professor of astronomy in the University of Cambridge; Professor Leonard E. Hill, director of the department of applied physiology, National Institute of Medical Research; Dr. G. A. K. Marshall, director of the Imperial Bureau of Entomology; Professor J. Arthur Thomson, Regius professor of natural history in the University of Aberdeen; Mr. H. W. A. Watson, lately chief conservator of forests, Burma; Mr. H. Wright, chairman of the executive committee of the governing body, Imperial College of Science and Technology, South Kensington. *K.B.E.:* Sir Philip Hartog, chairman of the education committee, Indian Statutory Commission. *C.B.E.:* Dr. E. W. Smith, honorary technical adviser to the area gas supply committee, Board of Trade. *O.B.E.:* Mr. T. P. W. Barty, lecturer in civil engineering, Gordon College, Khartoum, and municipal engineer, Khartoum; Mr. J. A. B. Horsley, electrical inspector of mines, Board of Trade; Professor W. M. Roberts, professor of mathematics, Royal Military Academy, Woolwich; Dr. F. B. Young, principal scientific officer, Admiralty Research Laboratory. *M.B.E.:* Mr. J. Haworth, general manager of the sewage disposal department and chief chemist and water examiner, Sheffield Corporation; Mr. H. W. Jack, economic botanist, Agricultural Department, Straits Settlements and Federated Malay States; Mr. H. G. D. Rooke, lately chief locust officer, Ministry of Irrigation and Agriculture, Iraq; Dr. A. Winstanley, junior inspector of mines, Board of Trade. *C.I.E.:* Mr. F. F. R. Channer, lately chief conservator of forests, United Provinces; Lieutenant-Colonel H. R. Dutton, lately principal, Prince of Wales' Medical College, Patna, and superintendent of the Patna Medical College Hospital, Bihar and Orissa; Mr. L. Mason, lately chief forest officer, Andamans; Mr. R. R. Simpson, chief inspector of mines in India. *C.V.O.:* Mr. Evelyn C. Shaw, secretary since 1910 to the Royal Commissioners of the Exhibition of 1851. *I.S.O.:* Mr. D. Keiller, head laboratory assistant, Imperial Institute of Veterinary Research, Muktesar, United Provinces.

SCIENTIFIC NOTES AND NEWS

YALE UNIVERSITY conferred on June 18 the doctorate of science on Dr. Edwin Grant Conklin, professor of zoology at Princeton University, and on Professor Charles Schuchert, professor of paleontology and historical geology at Yale University and curator emeritus of paleontology in the Peabody Museum.

THE doctorate of science was conferred at the commencement of the University of Pennsylvania on Alexander von Lichtenberg, professor of urology at the University of Berlin, and on Edward E. Allen, director of the Perkins Institution, Watertown, Massachusetts.

THE degree of doctor of laws has been conferred by the University of Indiana on Dr. V. M. Slipher and Dr. C. O. Lampland, of the Lowell Observatory at Flagstaff, Arizona.

DR. CHARLES SHEARD, of the Mayo Foundation at Rochester, Minnesota, received the honorary degree of doctor of science at the commencement exercises of St. Lawrence University.

MR. ORVILLE WRIGHT, Dayton, Ohio, co-inventor of the airplane, was awarded the honorary degree of doctor of science by the Ohio State University at the annual commencement.

AT the commencement exercises on June 10, the University of Arkansas conferred the degree of doctor of laws on Dr. Charles W. Webb, chief surgeon of the Clifton Springs, New York, Sanitarium and Hospital and president of the New York State Society of Surgeons.

DR. CHAS. B. DAVENPORT, of the Carnegie Institution of Washington, has been elected an honorary member of the Vienna Anthropological Society.

The Lucien Howe medal was presented to Dr. J. N. Adams, of Brooklyn, for distinguished research in the field of blind areas of the eye at the commencement exercises of the University of Buffalo.

Worcester Polytechnic Institute, at its commencement exercises on June 13, conferred the honorary degree of doctor of engineering on four of its graduates and upon the commencement speaker, Brigadier General Robert Irwin Rees, assistant vice-president of the American Telephone and Telegraph Company. The alumni who received the degrees are Harry Phillips Davis, vice-president of the Westinghouse Electric and Manufacturing Company; Henry Jones Fuller, partner in the Wall Street banking house of Alfred and Co.; Samuel Sumner Edmands, director of the School of Science and Technology at the Pratt Institute, Brooklyn, and Harrison P. Eddy, senior member of Metcalf and Eddy, consulting engineers, Boston.

Rear Admiral Richard E. Byrd will receive from President Hoover on June 20 a special gold medal of the National Geographic Society awarded for his distinguished contributions to the knowledge of Antarctica. On this occasion Admiral Byrd will make the first report on the research work of the expedition, and the motion pictures will have their first showing. The medal, especially designed, as Admiral Byrd already possesses the society's Hubbard Gold Medal, bears the inscription: "Richard Evelyn Byrd, rear admiral, U. S. N. He made distinguished contributions to knowledge of Antarctica, and was first to reach the geographical South Pole by air, November 29, 1929."

The American Geographical Society has awarded the Charles P. Daly Medal for 1930 to Lauge Koch, of Copenhagen, for his explorations in northern and eastern Greenland and his interpretations of the physical geography of that country. The David Livingstone Centenary Medal for 1930 has been awarded to José M. Sobral, director-general of the Argentine Bureau of Mines and Geology. Under Dr. Sobral's administration his bureau has for many years been productively engaged in a program of mapping and geographical and geological studies, especially in the Andean section of the country.

The Royal Aeronautical Society, London, held a dinner on May 30 after the delivery of the eighteenth Wilbur Wright Memorial Lecture by Mr. H. R. Ricardo. The presentation of a silver box suitably inscribed was made to Sir Richard Glazebrook to commemorate the twenty-first anniversary of the founding of the advisory committee for aeronautics, Sir Richard Glazebrook having been chairman of that committee since its foundation in 1909. Lieutenant-Colonel J. T. C. Moore-Brabazon was also presented with a small gift from the president and fellow-members of the council inscribed "To commemorate the twenty-first anniversary of his flight on the second of May, 1909—the first flight in a power-driven aeroplane piloted by a British subject in the British Isles." At the lecture which preceded the dinner the silver medal of the Royal Aeronautical Society was presented to Mr. F. H. Royce for his conspicuous work in the design and development of aircraft engines.

The committee on scientific research of the American Medical Association has made a grant of $1,500 to Dr. Robert Hegner, of the School of Hygiene and Public Health of the Johns Hopkins University, to aid him and his colleagues in the study of the host-parasite relations between man and his parasitic protozoa.

Professor John C. Slater, of Harvard University, is to be the new head of the department of physics at the Massachusetts Institute of Technology, succeeding Professor Charles L. Norton, who will in the future devote his full time to the direction of the division of industrial cooperation and research. This plan has the complete approval of the Harvard physicists as opening the way to increased cooperation between the physics departments of the two institutions. A conference of the Harvard physicists with Professor Slater and President-elect Compton was recently held, at which preliminary plans were made for such coordinated efforts. A new laboratory for research in physics and chemistry at the institute will probably be ready within a year and a half.

Dr. James Angus Doull, of the School of Hygiene and Public Health of the Johns Hopkins University, has been appointed professor of hygiene and public health at the School of Medicine of Western Reserve University, to succeed Dr. Roger G. Perkins, who is retiring at the end of the present year. Other appointments at the School of Medicine are: Dr. Donald E. Gregg, of the University of Rochester, instructor in physiology; Dr. H. A. Blair, of Princeton University, instructor in biophysics, and Dr. Ramon F. Hanzal, instructor in pathological chemistry. Dr. James W. Mull has been promoted from research fellow in biochemistry to be senior instructor in charge of biochemical research in obstetrics.

Dr. Alfred C. Redfield has been advanced to a professorship of physiology in the Harvard Medical School.

Dr. A. Lincoln Dryden, Jr., has been appointed associate in ecology at Bryn Mawr College for the coming year.

Mr. David B. Reger, of Morgantown, West Vir-

ginia, has resigned his position as associate geologist of the West Virginia Geological Survey, effective August 31.

DR. J. J. WILLAMAN, chief of the chemistry division of the New York Agricultural Experiment Station, will resign on October 1 to accept a position as biochemist with the Rohm and Haas Company of Philadelphia.

DR. E. V. ABBOTT has resigned as plant pathologist of the Agricultural Experiment Station, Lima, Peru, to accept an appointment with the Bureau of Plant Industry of the U. S. Department of Agriculture. Mr. Ralph H. Gray has resigned as horticulturist of the station, and is returning to the United States to enter commercial work.

DR. GEORGE ALLAN WORKS has resigned from the presidency of the Connecticut Agricultural College at Storrs and is returning to the University of Chicago. It is reported in an Associated Press dispatch that he was dissatisfied with the attitude of the state toward the college, as his policies called for the development of the academic aspects of the institution, while the state sought to develop it as an agricultural college.

AT the anniversary general meeting of the Royal Geographical Society, to be held on June 23, Colonel Sir Charles Close will resign the presidency of the society, and the council proposes as his successor Admiral Sir William Goodenough. It is also proposed to add Dr. Hamilton Rice to the list of vice-presidents. Brigadier Jack, one of the honorary secretaries, is to give up that post, and it is proposed to elect in his stead Dr. T. G. Longstaff.

DR. GEORGE K. BURGESS, director of the Bureau of Standards, was elected president of the National Conference on Weights and Measures at the concluding session of the twenty-third national conference recently held in Washington, D. C. Other officers elected were: Howard S. Jarrett, of West Virginia, *first vice-president;* Albert B. Smith, of Pennsylvania, *second vice-president;* F. S. Koolbrook, of the Bureau of Standards, *secretary,* and George F. Austin, Jr., of Detroit, *treasurer.*

DR. W. G. CROCKETT, professor of pharmacy in the school of pharmacy of the Medical College of Virginia, has been elected chairman of the Virginia section of the American Chemical Society and a member of the revision committee of the United States Pharmacopoeia.

DEAN CHARLES W. JOHNSON and Professor Henry A. Langenhan, of the University of Washington's College of Pharmacy, have been named to the U. S. Pharmacopoeial Revision Committee. Dean Johnson's term will be for ten years. At the recent convention of the American Association of Colleges of Pharmacy, held in Baltimore, Professor Langenhan was elected vice-president. Dean Johnson was president of the group in 1927.

DR. W. S. LEATHERS, professor of preventive medicine and dean of the Vanderbilt Medical School, has been elected a member of the board of scientific directors of the International Health Division of the Rockefeller Foundation.

MISS ALICE C. EVANS and Dr. Sara E. Branham, bacteriologists in the National Institute of Health (The Hygienic Laboratory), have been appointed delegates from the U. S. Public Health Service to the first International Microbiological Congress which meets in Paris from July 20 to 25.

MR. J. H. FLEMING, of Toronto, honorary curator of birds in the National Museum of Canada, and vice-president of the American Ornithologists' Union, attended the recent International Ornithological Congress at Amsterdam, as representative of the National Museum and the Government of Canada. Mr. Fleming is returning from Europe on July 1.

DR. C. E. HELLMAYR, associate curator of birds at the Field Museum, has gone to Europe on an ornithological research mission for the Field Museum. He has taken a number of rare bird specimens collected by the Crane Pacific Expedition, the Marshall Field South American Expedition, and others for comparison with type specimens in museums of Great Britain, France, Germany and other countries.

MR. J. R. VAN PELT, curator in charge of the division of geology and the mineral industries in the Chicago Museum of Science and Industry, returned from Europe recently following a two months' study of European technical museums. The new seven million dollar building for the museum is now in course of construction, and much of Mr. Van Pelt's time was devoted to the study of recent developments in museum buildings.

MR. WILLIAM R. BARBOUR, forester of the Tropical Plant Research Foundation, has returned to Washington after spending six months in Venezuela, Colombia and Nicaragua, making a survey of the timber resources of those countries. This year's work completes a three-year study of the Caribbean regions, made by the Tropical Plant Research Foundation with the cooperation of the Charles Lathrop Pack Forestry Trust, to determine the location and relative abundance of the woods of the tropics which might serve to replace or supplement hardwoods of the temperate zone. In connection with this survey, logs of fifteen species of Venezuelan woods have been sent to the University of Michigan to be given

thorough and practical tests to determine their suitability for use as substitutes for domestic hardwoods.

Professor Howard E. Simpson, of the department of geology of the University of North Dakota, delivered the address at the annual dinner of the North Dakota Academy of Science at Fargo on May 9, the State College Chapter of the American Association of University Professors acting as hosts. The subject of the address was "The Geological Story of the La Brea Tar Pits Retold."

The George Alexander Gibson Memorial Lecture was delivered at the Royal College of Physicians of Edinburgh on May 27 and 28 by Dr. William Sydney Thayer, emeritus professor of medicine of the Johns Hopkins University. Professor Thayer spoke on "Endocarditis."

The Halley Lecture was delivered at Oxford on May 30 by Professor A. S. Eddington, who took as his subject "The Rotation of the Galaxy."

The New Hampshire Academy of Science met at Henniker, New Hampshire, for its annual meeting on June 6 and 7. Officers for the year were elected as follows: *President,* Dr. Charles H. Dolloff, superintendent of the New Hampshire State Hospital: *vice-president,* Professor James W. Goldthwait, Dartmouth College; *secretary-treasurer,* Professor George W. White, University of New Hampshire; *chairman Publication Committee,* Mr. Frank H. Foster, Claremont, New Hampshire. The address of the retiring president, Mr. G. L. Cave, of the Brown Company, was on "Robert Calef, An Early American Scientist."

The Tenth International Conference of Chemistry will take place at Liège on September 14 to 20. The following agencies, committees and commissions will meet in the course of this conference: Council of the Union; General Assembly of the Union; Committee on Chemical Elements; Committee for the Reform of Inorganic Chemical Nomenclature; Committee for the Reform of Organic Chemical Nomenclature; Committee for the Reform of Biological Chemical Nomenclature; International Bureau of Physico-Chemical Standards; Committee on Thermo-Chemical Data; Committee on Tables of Constants, and the Committee on Physico-Chemical Symbols.

The Royal College of Physicians of Edinburgh proposes to celebrate on St. Andrew's Day, 1931, the two hundred and fiftieth anniversary of its foundation.

The University of Chicago has received anonymous gifts amounting to about $2,000,000 to build new laboratories for the departments of bacteriology and anatomy.

The Medical College of Virginia, Richmond, has received grants of $120,000, $40,000 from the Julius Rosenwald Fund and $80,000 from the General Education Board for the construction of a dormitory and educational unit for the school of nursing of the St. Philip Hospital. This is an institution for Negroes, owned and operated by the college.

The returns of the British Registrar-General for the first quarter of 1930 show that the birth-rate was 16.2 per 1,000 population, the death-rate was 13.5 per 1,000, while the infant death-rate was 77 per 1,000 registered live births. The birth-rate is the lowest recorded in any first quarter since the establishment of civil registration, except that of the first quarter of 1919, when, owing to the war, the rate fell to 15.7 per 1,000. The infant death-rate is the lowest ever recorded in any first quarter, being 16 per 1,000 below the average of the 10 preceding first quarters and 3 per 1,000 below that of the first quarter of 1928, the lowest reached in any earlier first quarter. The natural increase of population by excess of births over deaths was 26,725, as against a natural decrease in 1929 of 44,112. This difference is explained by the fact that influenza was not epidemic in the early months of this year.

The purchase of a 32,555-acre tract in South Carolina and of 5,180 acres in Colorado as migratory bird refuges has been approved by the Migratory Bird Conservation Commission, acting on the recommendation of the Bureau of Biological Survey of the Department of Agriculture. These areas will constitute the first bird refuges to be acquired by purchase under the migratory bird conservation act of Feb. 18, 1929, by which Congress authorized the expenditure over a 10-year period of nearly $8,000,000 for surveys and acquisition of lands for migratory bird refuges. The actual acquisition of the two areas will proceed immediately after July 1, 1930, when funds appropriated under the act become available to the department. The average price for these lands authorized by the commission is $1.13 an acre. The unit to be acquired in South Carolina is in the Cape Romain region, Charleston County, on the Atlantic seaboard; the other is in the San Luis Lake region, Alamosa County, Colo. Specialists of the Biological Survey have examined and appraised both areas from the standpoint of food resources for wild fowl and from other angles and have found them to be ideal refuges.

Under a special appropriation of $100,000 provided by Congress in the First Deficiency Act, the Bureau of Entomology will conduct special trapping experiments and extend its investigational work against the oriental fruit moth. Of this item, $80,-

000 provides for large-scale experiments with bait traps, to be carried on in two fruit sections, an area including 500 acres of peaches to be baited in each section. It is hoped that these experiments will answer the question which has long existed as to whether the use of bait traps over a wide area would give better results in the control of the oriental fruit moth than when a limited area of only a few acres is baited, in which case the surrounding unbaited area is in comparison very large. W. P. Yetter, Jr., has been placed in charge of the bait work. In addition to the maintenance of bait traps over large areas, important detailed experimental work is also contemplated. The localities selected for the work to be conducted during the season of 1930 are Cornelia, Georgia, and Vincennes, Indiana. The remaining $20,000 of this money is to be used to strengthen the work with parasites and insecticides and for a study of the ecology of the oriental fruit moth. The work with parasites and ecology is to be headquartered at Moorestown, N. J., under general supervision of L. B. Smith, and will constitute an enlargement of work already being conducted by Dr. H. W. Allen. The insecticide studies will be conducted at Vincennes, Ind., under the direction of Dr. F. H. Lathrop.

DISCUSSION

THE INTERNATIONAL CATALOGUE OF SCIENTIFIC LITERATURE AGAIN

THE proposal suggested by the Smithsonian Institution to revive the publication of the International Catalogue of Scientific Literature on a very modest but well-considered budget is a challenge to all scientists and librarians, and to all trust agencies which are spending good money for the promotion of research.

It is suggested that a revolving fund of $75,000 and one thousand library subscriptions of $50 for seventeen volumes will insure the enterprise. Whether or not this is enough is a detail. If this catalogue or something like it is an indispensable tool for research, as many first-class scientists seem to think, then any necessary amount should and probably can be had. If the catalogue is not needed, too much money is now being spent on it. Why waste more?

The Smithsonian raises this question plainly. Why ask the American government to continue to appropriate six or seven thousand dollars a year in the procrastinated hope of a resurrection, if the project is better dead? If it is needed, why procrastinate?

By putting the question the institution has deserved the thanks of all concerned. It is to be hoped that it will not let the matter rest until it has a square answer from all responsible parties. The history of the enterprise for ten years has been one of evasion, with preferential attention to other matters. Meantime a valuable asset of 2,500,000 manuscript titles, costing at least a million dollars, has lain unused and unusable, and another quarter of a million dollars more or less has been spent on half a million more titles, while marking time for responsible agencies to make up their minds or debtor nations to balance their budgets.

To a thrifty librarian the fact of these accumulated assets, together with the fact that the catalogue is a going concern with fifty to one hundred thousand dollars possible annual income, is the crux of the matter. The question raised by the Smithsonian is not the question of presenting a new project to be justified, financed and initiated, but whether perfectly good machinery worth at least $3,000,000 is to be scrapped, in an enterprise bound to be revived sometime, as Professor Armstrong, of the Royal Society, prophesies and as many scientific bibliographers in many countries are on record as believing.

It is at this point that the overture of the Smithsonian becomes a matter of practical business concern both to the research trust endowments and to the libraries. The research endowments are bombarded with bibliographical projects of varying method and degrees of merit. They aid or support a good many projects. They are deeply concerned as trust organizations to put their money where it will do the most good. Other things being equal they prefer to put it where one dollar will do the work of four. This seems to be a spot where one million, perhaps a quarter of a million, will do the work of four millions. If its usefulness merely averages with these other projects the endowments are likely to feel that its claims come first. It is here they can give the most bibliographical service with the least money. The proposition touches the libraries in a very similar way. If and when the matter is revived it will depend for financing, if not on the endowments, then on library subscriptions. If this machine is scrapped, when a new one is started either a $3,000,000 endowment must be had from promoters of research or a quadruple price charged to libraries.

This leads straight to the crucial question of whether the international catalogue is in fact a primary, essential or indispensable tool in such sense that it is bound to be revived sometime. It no doubt seems a futile and mortifying matter to those who have been deeply engaged in the problem for thirty years that they should have to rejustify and refight a

matter which was fought to the finish thirty years ago. But it is fair enough. It is not the only real bibliographical need of science. There are at least two other equally well-defined needs—abstracts and handbooks. Without disparaging the usefulness of these two other tools, it must be confessed that a good case is made by those who claim that something like the international catalogue is the essential and only indispensable tool among the three types.

A dispassionate general bibliographer must recognize that this is a conclusion towards which the whole history of bibliographical experience tends. The complete survey, in full title form, of the whole literature of any subject or group of subjects is the only solution of the main need of the student in research and in the higher learning, *i.e.*, completeness, and the best solution as to his need for a perspective.

The reason for this lies in the cooperative nature of advanced intellectual work. Intellectual progress, in whatever line, is based on gathering together the results of all previous intellectual work on a particular subject, large or small, surveying, integrating and building on this foundation. This collected and integrated material serves as a necessary basis both for those who, by teaching or reading, are at work turning scientific discovery into common knowledge, and for those who use the same integrated material as basis and point of departure for research effort to produce some new and useful variation, or contribution to the subject. If the research worker misses some result of work previously done, it involves at least the waste of having to do it over again, and it may involve loss of efficiency through the missing of some key fact.

The evidence that the full title method, as distinguished from the abbreviated or index method on the one hand and the annotated title or abstract on the other, is best, is that the title is just what bibliographical experience has evolved as the shortest description of the work or article which will serve. It is itself an abstract, made by the author himself to describe in the briefest way that he can devise what he considers the gist of the article. No other system of abstracts can pretend to describe a group of articles as well in the same number of words.

In short, bibliographical experience confirms the judgment that something "very like" this catalogue as to completeness is the essential, and full title method best, without prejudice as to variety in other details.

The obvious question at this point is, if the matter is so plain, in the nature of the case, in bibliographical experience, in the judgment of scientific bibliographers and users, in its financial aspects and in its waste eliminating and efficiency promoting character, why has the catalogue not been revived long ago?

There are in fact reasons which might well have hindered the most worthy of enterprises.

In the first place, there were the war debts. The temper of the congress of regional bureaus in 1922 was so decided in favor of revival that the committee appointed by it might likely have made progress in getting support from the respective governments for the overhead editing and printing also if it had not been for the war debt situation. The committee found on inquiry what has since been confirmed under renewed inquiry, that under the debt repayment condition it has been difficult enough to keep up on the work of the national bureaus and quite impossible to ask to add to the post-war budgets for overhead and printing in view of the sharp watch and criticism by creditor nations on all budget increases.

Another reason thought to be decisive by Professor Armstrong is the growing passion for specialization among scientists which makes them indifferent to the seeing-as-a-whole aspects, promotes a scramble for special privilege and blinds to the economies of wholesale and machine production in bibliography, as against desultory special bibliographies.

Again, the initial demand for a cool $1,000,000 ($75,000 per year for fourteen years) for publishing arrears was in fact a major deterrent. It is a sum big enough to give pause to any project and demand full justification before proceeding. The Smithsonian now proposes to cut this sum out altogether, leaving arrears to the future. At any event this problem of arrears could be fairly provided for by simply filing the cards as they are and organizing a modern library card and photostat information service, at a cost of 10 per cent. of the printing estimate.

Finally, there is the matter of means, or rather of connecting with the means, for it seems obvious that there is no serious lack of money for approved scientific bibliography. Very much more money is now spent for this in a desultory and competitive way than would be needed for a comprehensive wholesale handling.

There seems little doubt, if one can judge by the course of other events, that, if the above statement of the situation is sound, some agency for the promotion of research will be glad to furnish at least the minimum means. If it is true that the catalogue fills an essential need, in the best way, on a wholesale scale, which insures the most economical production and low cost to the user, by eliminating wasteful and ineffective casual effort, there is little chance that it will not find reasonable provision, if it can get an adequate hearing.

The problem is, therefore, to get the facts stated in a convincing way and presented by those who have the confidence of the promoters of science.

Of course the Smithsonian itself has its own prestige. It might use this prestige to induce the two great institutions organized for the promotion of cooperation in intellectual work, the League Committee and the National Research Council, to take up this particular question for definite consideration on its merits and to consider the whole situation of the bibliography of science in a broad way with view to inducing the cooperation or amalgamation of existing enterprises. This would reach one of two results: either the promotion of this project in its suggested form, or a modified form, perhaps a highly modified form, in which latter case it would inevitably lead to some project viewing the whole field of bibliography as one; or on the other hand, it would produce a responsible opinion against the catalogue which would justify the Smithsonian in abandoning the project and refusing to apply for further appropriations.

Scientific bibliography has the very high honor in bibliographical history of having been the first to conceive and to carry out on a large scale in the international catalogue the seeing-as-a-whole aspect of things which the modern school of psychologists is now exploiting. It would be an even greater honor if it should lead the promoters of research generally to apply the comprehensive method to other large fields.

Ernest Cushing Richardson
Library of Congress

WHAT IS CONTROL?

We fail to understand by what authority, or process of reasoning, Professor Woodworth[1] would limit the use of the term *control* to "definite conscious action of a rational being, something done by man for his own benefit . . . always something that carries out his will."

The dictionaries define control, *v. t.*, in part as follows: "to exercise a directing, restraining, or governing influence over; direct; counteract; regulate" (Standard); "to exercise control over, in restraining or checking; to subject to authority; direct; regulate; govern; dominate" (Century); "to exercise restraint or direct influence over, to dominate, regulate; hence to hold from action, to curb, subject, overpower" (Webster). One could scarcely formulate a truer picture of the present-day aims of economic entomologists, with reference to our insect enemies. Nowhere do we find a definition that restricts the term as postulated by Professor Woodworth. *Uncontrolled,* to our way of thinking, means a condition where control by man or by any other factor is not sufficient to restrain or dominate.

[1] Science, 71: 388, April 11, 1930.

Professor Woodworth apparently objects to including under control the action of parasites and predators. Certainly a parasite or predator which destroys a noxious insect is "counteracting," "curbing" and "exercising a restraining influence on" the development of that species. Whether or not it is conscious of what it is doing, or is carrying out its own will, makes no difference in the end result.

Is it not time that biologists, at least, should recognize that man is an animal and a part of nature, by discarding the term artificial for all his relations to the rest of the organic world? In a very real sense, man's fight against his insect enemies is as natural as that of a parasite or predator. Until we are positive that "definite conscious action" is found only in the behavior of the human species it may be unwise to emphasize unduly our separation from the rest of the animal kingdom.

We also fail to follow the connotation that remedies are necessarily eradicative. The term remedy seems to be used at present chiefly to designate pharmaceutical preparations or medicines used for the cure or relief of diseases or ailments. These, we are too sadly aware, are generally far from being eradicative. Remedies, like treatments, imply that the trouble which they are aimed to correct has already begun. Preventive measures, on the other hand, are anticipatory, and are aimed to ward off, or stop the trouble from happening, by the application of previous measures. Remedies, treatments, preventive measures, parasites and predators—all "counteract" or "restrain" the pests against which they are used, and therefore control seems to us to be the best general term.

We would include under the general comprehensive term insect control all adverse operations and ecological conditions that make life hard for insects, that tend to kill them or to prevent their increase in numbers or their spread over the earth. As so defined, insect control may then be classified as follows:

A. Applied control: measures that depend upon man for their application or success, and can be influenced by him to a considerable degree.
  1. Chemical control: the use of insecticides and repellents, substances that kill insects by their chemical action or ward them off by their offensiveness.
  2. Physical or mechanical control: special operations that kill insects by their physical or mechanical action.
  3. Cultural control: regular farm operations performed in such a way as to destroy insects or prevent their injuries.
  4. Biological control: the introduction, encouragement, spread and increase by human aid of

predacious and parasitic insects and other animals and insect diseases.

5. Legal control: the control of insects by controlling human activities.

B. Natural control: measures that do not depend upon man for their continuance or success, and can not be greatly influenced by him.

1. Climatic control: the restraining influence of cold, heat, winds, storms and other adverse weather factors.
2. Topographic control: the restraining influence of natural barriers, such as oceans, rivers, mountains, unfavorable vegetation or soil.
3. Biological control: the operation, without human aid, of the parasites, predators and insect diseases naturally present in any region.

We believe that there is a perfectly good reason why "the term control . . . has finally practically displaced the older term" and why less than 5 per cent. of recent writers on insects and fungi are using other terms in place of it.

C. L. Metcalf

University of Illinois

The appearance of an article entitled "What is Control?" by Professor C. W. Woodworth in a recent issue[1] raises an interesting point. If natural control, as a term, be discarded, as suggested, what substitute may best be employed in less technical bulletins of experiment stations and the like to designate those factors of nature which exert a restraining influence upon organisms, such as injurious insects and fungi, which are the subjects of such publications?

Chapman, Graham and others have developed the term environmental resistance, which has been defined[2] as "the sum of all the factors in an environment that tend to reduce the rate of insect multiplication." These factors are physical, biological, etc. This term, environmental resistance, which is, perhaps, more inclusive in scope than the connotations associated with natural control by some, might well be used exclusively in place of the latter; "natural control," however, is often useful. The impression has been gained that natural control, as a term, need not be abandoned on the basis of the argument of the article mentioned.

After a lucid and rather satisfying explanation of how "control" has supplanted such medical expressions as "remedy" and "preventive," Professor Woodworth concludes that "natural control . . . should disappear from the literature of entomology" because, as a term, it is considered self-contradictory and the equivalent, etymologically, to non-control. This objection is based upon the concept that, since the influence of those factors which, properly, may fall into this category is exerted quite independently of man, no control has operated, *i.e.*, control is not control unless initiated, directly or indirectly, by man (or by some other intelligence in the universe acting for the benefit [?] of man), and control "is always something that carries out his will."

This limited and anthropomorphic construction of control is not supported by the opinions of certain lexicologists. Two dictionaries, taken at random, define control as "a check," "a restraint," "the power of keeping checked," "a regulation." No express statement that only man (or a superhuman entity) can exert such an influence is usually to be found; indeed, "one who or *that which* controls"[3] has been noted.

In the article cited, it is further stated that "uncontrolled . . . is almost universally expressive of the action of nature where a control by man is not exercised." If it be granted that this usage may hold generally—though, strictly, it does not hold exclusively, and one may conceive of non-human limiting factors that may not, or have ceased to, operate—it still need not invalidate the term natural control. Philological studies have shown the essential plasticity of a living language, as witness *sanguine,* originally *bloody,* then *abounding with blood,* finally *cheerful, hopeful, confident.* As the remark of a colleague implies, the points made against the use of the term natural control might hold with at least equal force (or lack of force) with "natural selection." The article cited does not challenge the fitness of this particular term, which has achieved virtual universal acceptance and which may be said, conservatively, to be good usage.

If "control" may be used as it has been defined: a check[4] or a restriction, it then would seem not excessively arbitrary to add "natural" (belonging to nature, not artificial) to obtain an expression descriptive of such natural factors as parasites, predators, lethal temperatures, fluctuations in the food supply, *et al.,* which—from the view-point of man—do much to limit the injuriousness of certain insects, fungi, etc. In view of the usages of indirect and direct control, chemical and biological control, etc., it should not be amiss to retain, as an antithetical expression for artificial control, the simple and reasonably self-defining term, natural control. It is in just this sense that such eminent entomologists as W. R. Thompson and F. Silvestri have used natural control.

Raymond L. Taylor

Bar Harbor, Maine

1 Science, 71: 388, April 11, 1930.

2 S. A. Graham, "Principles of Forest Entomology," p. 32, McGraw-Hill Book Company, New York, 1929.

3 Funk and Wagnall's Desk Standard Dictionary, p. 188, 1915.

4 Incidentally, "check" has at least two meanings for which "control" is used synonymously, *viz.* (1) a restraint, (2) the untreated unit or phase in an experiment.

THE SECOND CAPTURE OF THE WHALE SHARK, RHINEODON TYPUS, NEAR HAVANA HARBOR, CUBA

IN 1928 we put on record[1] the capture of a thirty-two foot whale shark on November 20, 1927, at Jaimanitas, a fishing village in the suburbs of Havana, about five miles west of the mouth of the harbor. Now we have the data for the taking of another at Cojimar Bay about as many miles to the east of Havana, and interestingly enough the new specimen is of about the same size—the former thirty-two feet, the present one thirty-four feet.

On March 10, 1930, a Havana paper contained a notice of the capture of a gigantic "Pez-dama" or checkerboard fish on that day off Cojimar. Investigation confirmed the account. The Havana papers during the next few days printed a picture of the fish drawn up on the beach and gave accounts of its capture which on investigation were found to give the facts. All available data from the papers and from talks with the fishermen have been used in writing this notice.

The accounts, which seem reliable, are that this or another fish had been seen in the open sea off Cojimar for at least three years, and efforts had been made to capture it. So well known was it that the fishermen had dubbed it "El Elefante" from its huge size. That the presence of this fish off Cojimar was common knowledge is attested by the fact that a Spanish merchant, Sr. José V. Fernández, of Havana, had subsidized a crew of fishermen under José González to watch out for and to capture the fish. They had been provided with steel cables, empty metal casks or "drums" and specially made harpoon lines and harpoons. The necessity for the latter will be understood when it is recalled that the skin of *Rhineodon* is from three to four inches thick, and not penetrable by the ordinary harpoon.

The method of capture seems to have been somewhat as follows. Two gasoline launches managed to get a slip noose of small steel rope over the shark's head and around his middle—over the pectoral fins and just in front of the dorsal as is shown in a photograph. The wire cable was then drawn tight, confining the pectorals and hampering the fish's activity. Since this attack took place out on the open sea, in water of some depth, it was necessary to hinder the shark from diving or at least to locate it when submerged. This was effected by affixing (how we have not been able to ascertain) empty metal barrels or drums to the cable. This done and the whale shark being more or less held at the surface, two or three harpoons were thrown into it just back of the head and above the gill region. With *Rhineodon* thus doubly held to the boats, about fifty shots were fired into its body in the endeavor to kill it. We have a photograph in which the wire rope and the harpoons are plainly visible.

The fish does not seem to have offered much resistance to this treatment, and was finally towed and driven from the open sea into Cojimar Bay and stranded in shallow water. A crowd quickly collected and an effort was made to pull the gigantic fish up on the beach. But since the monster was thirty-four feet long and had an estimated weight of nine tons, the utmost efforts of forty men were not sufficient to effect this, and it was finally achieved by putting the fish on a wooden framework (allied to the "stone boat" of New England) and by dragging this up by means of a winch or crane. The fish was still alive when brought to the beach and died some twenty-four hours later—presumably from loss of blood resulting from lance thrusts in the gill region. There seems to be no other method to compass its death, unless by the use of a bomb-harpoon.

The huge fish was finally drawn on the shore where it was covered with an awning to protect it from the sun. Here it was visited by great numbers of people not merely from the countryside but from Havana. The picture published by the Havana *El Diario* was made of the fish under the awning. Later the skin was removed and taken to Havana where it is being mounted for exhibition.

As in the case of the other Havana specimen this fish seems to have come near shore in pursuit of schools of sardines, on which the fishermen allege that it feeds. This, as noted in our former paper, coincides with the other definite information which we have about its feeding habits in the western Indian Ocean.

This fish, a male as was the other Havana specimen, offered no effectual resistance to capture, but seemed very sluggish—even stupid. This indeed seems characteristic of the whale shark. All other specimens taken in the Florida Straits have put up no fights at all, in fact have done nothing save drag the attacking boats around. However, the three captured on the Florida coast (the fourth came ashore dead) have been taken in shallow water where they could not effectively exert their strength. But the two taken near Havana have made no more resistance than did the Florida fish, notwithstanding the fact that they were taken in the open sea, in water of at least moderate depth—water by no means so shallow as to hamper the fish. These two did no more than drag the boats around with them, as the others have done.

In conclusion we may add that the capture of this new specimen of *Rhineodon* is by no means surprising. This conclusion is arrived at because the capture of one other Havana and of four Florida

[1] E. W. Gudger and W. H. Hoffmann, "The Whale Shark, *Rhineodon typus*, near Havana Harbor, Cuba; the Fifth Record from the Straits of Florida," American Museum Novitates, 1928, no. 318, 7 pp., 4 figs.

specimens have been recorded from this general region. And secondly, because the head fisherman of the crew that caught the first Havana specimen reports that after the capture of this fish he had two months later seen in the same locality and had tried to harpoon another huge spotted shark. The same reports come to us now for a second specimen in the waters of Cojimar. Then again we have seen recent newspaper accounts (unconfirmed) of a specimen seen off Bimini, Bahamas.

It is a matter of regret that the demise of Sr. Fernández took place some months before the realization of his dream for the taking of this great shark, the search for which he had maintained for many months. However, the mounting of the fish seems to be going forward under the direction of his widow and of the head fisherman and it is to be hoped that the exhibition of the mounted fish may bring in sufficient returns to recoup at least some of the expenses of its capture and preparation.

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QUOTATIONS

REMARKS ON THE HISTORY OF COSMIC RADIATION[1]

IN previous articles I have never sought to assign the origin or history of the speculative ideas about atom building in cosmic processes—a very ticklish thing to do, since during the past twenty years this question has aroused general interest. But if the historian of this domain can find anything useful in it, I will be glad to contribute my own knowledge of the history of the subject.

In the year 1904, when I was engaged in the study of certain rare ores for their uranium content by the action of radioactivity, Professor F. R. Moulton, of the University of Chicago, came to me with the statement that even if the sun were originally of pure uranium it could not have given up as much energy as he would regard as necessary for a minimum of the life of the sun, and that, therefore, it was necessary to postulate a store of cosmic energy from a previously unknown source for the stellar energies.

Now this source had already been found, although I did not, at that time, fully appreciate it; the interchangeability of mass and energy was demonstrated in 1901 for special cases by the experiments of Kaufmann, and the discovery of radiation pressure some years before was also of great importance. A few years later (1905) Einstein discovered this interchangeability as a consequence of the special theory of relativity, and from this time on this theory was available to any one who, like Professor Moulton, was seeking a new source of energy for the continued existence of the life of the celestial bodies. Certainly, for something less than ten years it was a theme of general table conversation at the University of Chicago. As soon as the Mosleyian relations (1913–14) and the existence of the isotopes were discovered, atom building within the stars, accompanied by a change of the superfluous mass into radiation, was considered as a source of stellar energy. Harkins[2] explained in detail this loss of mass, or packing effect, in the atom building process. I mentioned this fact in the first edition of my book "The Electron."[3]

That this phenomenon is not sufficient to explain the energy of the universe was shown later on. In *Nature* (1917) Eddington mentioned the idea of the annihilation of matter by collision and the complete superposition of the positive and negative electrical fields, and ascribed the idea to Jeans.[4]

Certainly by the year 1915 the idea of the building of the elements from hydrogen as a source of universal energy was prevalent, and in 1917 the total destruction of mass as a more active source found its way definitely into the literature, and was familiar at other universities than Chicago, since these ideas are obvious consequences of the Einstein equations (1905) and the known existence of isotopes (hydrogen with the atomic weight 1.008 instead of 1).

In our conversations at Chicago W. D. MacMillan constantly held out for the view that a still further step forward should be taken and that the idea of the "running down of the universe" should be given up by the assumption that atom building went on in space by the condensation of radiation into atoms. He discussed this idea with me in detail in the year 1915, and in July, 1918, he published it in full.[5] Any one who is interested in the history of this subject should read MacMillan's other articles,[6] since this investigator, on the theoretic side, is the foremost representative of the idea of the development of cosmic energy by the process of atom building.

These three ideas, first, atom building from hydrogen; second, the radiating away of mass, and third, the condensation of radiant energy into atoms, are

[1] Translated from the *Physikalische Zeitschrift*, Nr. 6, March, 1930.

[2] *Phil. Mag.*, 30: 723, 1915.

[3] P. 203, 1917.

[4] *Nature*, 70: 101, 1904.

[5] *Astrophysical Journal*, 48: 35, 1918.

[6] SCIENCE, 62, July 24 and August 7, 1918.

the three hypotheses for which we have obtained partial experimental proof.

Heretofore I have not tried to assign priority to any one with respect to speculative ideas, since we have considered it our problem to show how far our experimental results were of significance to these now familiar ideas. We made a small step forward in giving a quantitative proof for the cosmic origin of the radiation in 1925, in that the longest wave-length observed, according to our method of calculation, agreed with the Einstein equation corresponding to the building of helium out of hydrogen; and last winter (February, 1927) we found clear and authentic proof that this and other atom building processes are actually the source of the cosmic radiation. We proved further, contrary to all previous assumptions, aside, perhaps, from the assumption of MacMillan, that the atom building process does not occur in the stars, but in the depths of interstellar space.

If there is any one besides Einstein who was a pioneer in the development of the theoretical ideas for which we have found experimental proof it is W. D. MacMillan. Any one who since 1918 may have sought to write the history of the atom building processes should have given him a deserved recognition.

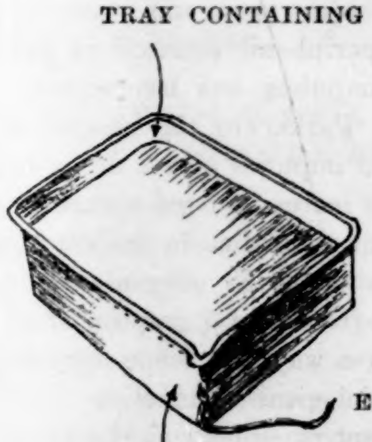
ROBERT A. MILLIKAN

SCIENTIFIC APPARATUS AND LABORATORY METHODS

AN APPARATUS FOR HANDLING PARAFFIN RIBBONS

THE need for a convenient means of handling several paraffin ribbons at one time was the incentive for contriving the following piece of apparatus.

A good-sized photographic tray was fitted into the top of a metal box containing an electric heater and a thermostat. The tray was filled about half full of water. Water at a temperature of approximately 30° C. spreads the ribbon out smoothly yet is not so hot as to make the paraffin soft enough to be inconvenient to cut with small scissors.



The paraffin ribbons are cut to about the length of the tray and transferred to the water with a pair of tweezers. The bath is large enough so that several long ribbons can be spread out side by side. With the aid of a strong light and a hand lens the unwanted parts of the ribbon can be detected and removed from the bath. Then the ribbon is cut to proper lengths, the fixative-covered slide is slipped under the section and the piece of paraffin ribbon floated to the exact position.

The tray may be removed and a plate placed on top or the whole box turned over, resulting in an ordinary constant temperature embedding table, which can be used for drying slides and for softening the paraffin before dissolving the ribbon in xylol.

Briefly summed up, this piece of apparatus does the following: (1) Spreads the paraffin ribbon out flat and smooth. (2) Holds several long ribbons. (3) Undesirable parts of the ribbon can be detected and removed. (4) The paraffin ribbon can be easily cut to desired lengths. (5) Provides a convenient method for placing the piece of ribbon on the slide. (6) Turned over it functions as a large embedding table.

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SPECIAL ARTICLES

THE METABOLISM OF THE LOCAL EXCITATORY PROCESS AND OF THE PROPAGATED DISTURBANCE IN NERVOUS TISSUES[1]

IN an earlier paper I tried to show that the chemical changes produced in a tissue by means of an electrical stimulation differ from those caused by the physiological conduction of excitation waves. I was at first brought to this opinion by the following facts. Nearly all investigations of the metabolism of the excitatory process in the nervous system were carried out in such a manner that the part to be investigated was stimulated directly. Parker alone for methodical reasons stimulated the nerve outside of

[1] Some of these results have been communicated in an evening lecture at Woods Hole and published in the *Collecting Net*, Vol. IV, No. 8.

the chamber in which the CO_2 output was measured. And whereas nearly all other authors had found the gas exchange increased from 50 to 300 per cent., Parker obtained only an increase of about 14 per cent. This made me suspect that the differences between these results were caused by a difference between the metabolism of the local excitatory process and that of the propagated excitation waves, and induced me to pursue the question further.

One of my collaborators, v. Ledebur, made comparative investigations of the oxygen intake and the CO_2 production of the isolated spinal cord of the frog, stimulating with induction shocks either directly or by way of reflex through the sciatic nerve. In the first case he observed a strong increase of the gas exchange, while the reflexive, that is, the physiological, stimulation produced no increase, or only an insignificant one, in the normal spinal cord. In the organ poisoned with strychnine, evidently in consequence of the greater spreading of the excitation, the increase was larger, but still much less than with direct stimulation. It seems to me impossible to explain this in another way than by fundamental differences between the effects of local stimulation and of physiological conduction.

If this is the case with the central nervous system, it must be expected that the peripheral nerve behaves in the same manner. Therefore I performed in the Marine Biological Laboratory of Woods Hole and in the Physiological Laboratory of Breslau a number of experiments concerning the influence which is exerted on the oxygen intake of different nerves on the one hand by the local excitatory process and on the other hand by the propagated disturbance. I used for this purpose a microrespirometer, which allows either a part of the organ situated inside of the respiratory chamber or a part left outside of it to be stimulated by electrical shocks. Using the leg nerves of bull-frogs I was surprised to see that the increase of the gas exchange effected by direct electrical stimulation was incomparably less than I had observed in German frogs. Nevertheless, in a few experiments the increase of oxygen intake was distinct by stimulation inside the chamber, while the conducted excitation produced by the stimulation outside of the chamber did not cause any increase at all. This, however, can be shown much more distinctly with European frogs, which, as mentioned above, have a much larger increase of gas exchange when stimulated. While this increase with direct stimulation amounted to between 50 and 80 per cent., the propagated disturbance again did not cause any measurable increase. The derivation of action currents at the end of the experiments proved that the conductibility had been preserved. From these experiments certainly it must be concluded that the chemical changes produced locally by electrical stimulation differ distinctly from those caused by physiological excitation.

This is also made evident by experiments performed in a similar manner on the isolated spinal cord of the dogfish. Here too the stimulation outside of the respiration chamber did not increase the oxygen intake, while the stimulation inside of it produced an increase of from 40 to 75 per cent. If the movements of the tail on stimulating the spinal cord are taken as a test of excitability, the latter seems to have disappeared after one hour. Therefore in this experiment the physiological conduction was perfectly suspended, while the local excitatory process produced by artificial stimulation was preserved. This shows that the usual method of judging the local excitability by the reactions of a remote end-organ is quite subject to error.

There are a number of observations which agree very well with these ideas. Parker carried out an ingenious experiment about the gas exchange of unsevered nerves. He isolated a large part of the vagus nerve of snakes without cutting it. Then the neck of the animal was bent towards the heart and the vagus looped in the opposite direction. This loop was inserted into the respiratory chamber, where the CO_2 output was measured. No change of this CO_2 output was noted when the nerve was separated from its central and its peripheral connections and thus the passage of all impulses was interrupted. This observation also led Parker to the conclusion that the passage of normal impulses over a nerve does not call for an observable increase of gas exchange. Perhaps also the peculiar difference in the effect of the electrical stimulation upon the oxygen consumption of the nerve in bull-frogs and European frogs may be explained in such a way. It seems very improbable that the physiological conduction should be fundamentally different in animals so closely related; it is much easier to assume that the effect of artificial stimulation differs because of some unimportant structural peculiarities.

The artificial electrical stimulation of an organ has generally two different effects: changes starting the excitation waves and containing the processes of metabolism observable under physiological conditions; besides these changes the stimulation effects further local chemical processes which depend on the intensity of the current and have nothing to do with the processes taking place under normal conditions.[2]

[2] I think these views agree with the ideas expressed by Dr. Lillie in his review of my lecture (*cf.* the *Collecting Net, loc. cit.*).

For the gas exchange both kinds of changes differ vastly; the large increase of the oxidation, up till now regarded as the expression of the physiological excitation of the nervous system, is only an artificial product of the electrical stimulation. This must not refer to all chemical processes. Some years ago I found with my collaborators that the isolated central nervous system of the frog consumes sugar from the surrounding solution and that this consumption of sugar, especially of glucose, is very much increased by electrical stimulation. Now I performed a new series of experiments, where the sugar consumption was compared when the spinal cord was directly stimulated and when it was stimulated by way of reflex through the sciatic nerves. In contrast to the observations on the gas exchange the result was quite the same in both cases, as well in the normal, as in the strychninized organ. Therefore the increase of sugar consumption is no artificial product of stimulation, but a process really conditioned by the physiological excitation. As the sugar consumption produced by way of reflex is accompanied by only a small increase of oxygen intake, we must conclude that the main part of the sugar does not disappear through oxidation.

I can not close without thanking once again Dr. Jacobs for the extraordinary hospitality shown to me at the Marine Biological Laboratory in Woods Hole.

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THE OCCURRENCE OF A PELLAGROUS-LIKE SYNDROME IN CHICKS

A PELLAGROUS-LIKE syndrome in chicks has recently been obtained at this laboratory in an experiment which was originally designed to throw added light upon an unusual type of leg paralysis occasionally encountered in chicks fed semi-synthetic rations.

The external manifestations of this nutritional disease appear chiefly at the eyes, at the mouth corners and upon the feet. The edges of the eyelids become granular and contract so that vision is restricted. Later, a viscous exudate is produced which causes the eyelids to stick firmly together.

Crusty scabs appear at the corners of the mouth. These gradually enlarge and may even spread so as to involve the margins of the skin around the nostrils and underneath the lower mandible. The skin upon the bottoms of the feet and between the toes peels off. Afterwards, small cracks and fissures appear at these points. These enlarge and deepen so that chicks affected are sensitive to walking.

Feathering is retarded, and the few feathers produced are rough and staring. There is, however, no loss of down or feathers comparable to the loss of hair obtained in pellagra in rats.

Post-mortem examination of chicks that die almost invariably shows the presence of a pus-like substance in the mouth and of a grayish-white exudate in the stomach. The entire intestinal tract is almost entirely devoid of undigested food residues. The small intestines lack tonicity and appear atropic.

The liver is found frequently to vary in color from a faint yellow to a deep dirty yellow, and occasionally it may show hypertrophy. The kidneys reveal a tendency to enlargement and appear grayish-white or inflamed and hemorrhagic.

This syndrome first appeared in a group of White Leghorn chicks when about three weeks of age which were fed a normal diet except for the use of Merck's powdered egg albumin in place of the more common protein of animal origin. At six weeks of age the few chicks remaining averaged 128.4 grams in weight and the mortality was 72 per cent.

The substitution of purified casein for the egg albumin in the basal ration delayed the onset of the pellagrous-like symptoms but gave no improvement in growth. Granulation of the eyelids and encrustation of the mouth corners were less severe in this group, but most of the chicks developed feet conditions just as bad as those in the egg-albumin group. The average weight of these chicks at six weeks was 119.0 grams and the mortality was 36 per cent.

The addition of 2.5 per cent. of autoclaved yeast to the purified-casein basal diet improved growth, prevented granulation of the eyelids and encrustation of the mouth corners but did not prevent entirely the occurrence of scaly, cracked feet. Five per cent. of autoclaved yeast completely prevented all these conditions and produced still better growth. The best growth, however, was obtained by the addition of 10 per cent. of autoclaved yeast to the purified-casein basal diet. At six weeks, the average weight of the chicks in this group was 420.6 grams, a weight 33.4 per cent. greater than the normal average weight for White Leghorn chicks six weeks old used at this laboratory. No mortality was obtained in this group or any of the other groups which had received autoclaved yeast.

The data obtained in this experiment demonstrate the intense requirement of another species for the vitamin or vitamins present in autoclaved yeast, commonly called vitamin B_2, vitamin G or the P–P factor, and indicate that the chick may be a more suitable animal than the white rat for determining the quantity of this vitamin present in food-stuffs.

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THE EFFECTS OF X-RADIATION ON COTTON

IN the summer of 1927, during the period when Dr. H. J. Muller was obtaining the first of his notable results on the extent of transgenation induced by X-rays in *Drosophila* and at his suggestion, a population of cotton plants was grown in the greenhouses at the University of Texas to provide material for parallel experiments in the case of a plant species of commercial importance. This original population was produced from seed of a single plant in a highly selected strain of "half and half" cotton supplied by the seed firm of Sumner and Company at Vernon, Texas. The experimental material was, thus, in as relatively homozygous condition as can be obtained in varieties of cotton grown under controlled breeding conditions.

Flowers of these greenhouse plants were emasculated the evening preceding anthesis, and the unopened anthers placed in gelatin capsules. The X-ray treatment was applied the next morning, and the pollen, which by this time had escaped from the anthers, was immediately placed on the stigmas of emasculated flowers. The control flowers were manipulated in the same manner in order to compare the setting of fruit in the two series. It was not considered necessary to keep the anthers of each flower separate for treatment since the individuals in the population were sister plants all having as their parent a single plant of highly inbred origin. The X-ray exposures used were 4, 8, 12, 16 and 25 minutes with a set-up of 50 kv., 5 ma., target distance of 10 cm and an aluminum filter. Seventeen mature fruits, yielding 311 seeds, were obtained from X-rayed pollen × untreated eggs and a larger number of fruits and seeds from the pollinations involving untreated pollen. In the spring of 1929 one half of the seeds from X-rayed pollen were planted in pots in the greenhouse at Berkeley, and from this number twenty-one plants were grown to maturity. The other half were planted in the open, but failed to reach the fruiting stage on account of unfavorable weather conditions. Plants from untreated pollen were grown as a control. It was originally noted that in the seventeen fruits obtained from treated pollen there was a decrease in number of seeds per fruit as the dosage became heavier. A further evidence that sterility was a by-product of the treatment is seen in the production of only twenty-one plants from over 150 seeds.

In external morphology many of these twenty-one plants were altered as compared with sister plants from untreated pollen. Among the more striking of these alterations in character expression was the presence of twisted and deformed stigmas, anastomosing leaf veins, peculiarities in leaf shape, fasciated and enlarged stems, incomplete flowers and dwarfness in habit. Only twelve of the twenty-one plants produced fertile fruits during the 1929 growing season, and seeds from two of these were empty. Three plants were obtained from seeds of the 25-minute dosage. One of these died before reaching maturity, and the other two failed to produce mature fruits.

An examination of the seeds of the ten fertile plants with perfect seeds shows a marked degree of variation as compared with seeds from the control plants. The uniform size of "half and half" seeds is one of the striking features of this variety of cotton, and in a quantity of seed it is hardly possible to select noticeably large or small seeds, so uniform is this character. Of the ten fertile plants mentioned above three produced seeds very much larger than the average size in the control. More striking, however, was the variation in the character which concerns the attachment of the lint in the mature seed. In "half and half" every fiber is attached to the seed at maturity, and must be pulled away in ginning. Two plants of the ten produced seeds from which the lint was entirely free at maturity, resulting in what might be termed "naked seeds." Three plants produced seeds which showed this character to a noticeable extent. In none of the control plants did this character appear.

Cytogenetic analysis of this X_1 generation is in progress and X_2 progenies are being grown. The information in hand suggests that quantitative and qualitative alterations in the hereditary material may readily be induced in cotton by treatment with X-rays. On the basis of these initial experiments it is anticipated that evidence may ultimately be forthcoming which will parallel the results obtained in this laboratory as to the deep-seated effects induced by high frequency radiation in tobacco, another species of economic importance.

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BOOKS RECEIVED

KIDSON, EDWARD. *Meteorology.* Reports on the Scientific Investigations of the British Antarctic Expedition, 1907–1909. Pp. 188. 18 figures. 33 tables. Government Printer, Melbourne, Australia.

MAERZ, A., and M. REA PAUL. *A Dictionary of Color.* Pp. vii + 207. 56 plates. 3 tables. McGraw-Hill. \$12.00.

WULF, THEODOR. *Modern Physics:* A General Survey of its Principles. Translated from second German edition by C. J. Smith. Pp. xi + 469. 202 figures. 34 tables. Dutton. \$10.00.